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ISAAC NEWTON, 1642–1727

CHRISTMAS DAY is the three hundredth anniversary of the birth of Newton, an event which, were it not for the conflict and chaos of these times, would have been celebrated on a world-wide scale, and marked by an international gathering of representative men of science in London and probably also at Grantham. In the present circumstances, we have to be content with more modest proceedings. Thus the Royal Society made its anniversary meeting on November 30 a Newton celebration, the Physical Society arranged a special lecture given in London and repeated at Cambridge, and a gathering at Grantham was addressed by Sir Henry Dale, president of the Royal Society.

It is on a similarly limited scale that the event must be marked in NATURE. Newton's life and work are surveyed by Prof. S. Brodetsky, of the University of Leeds. We also have the privilege of printing the addresses delivered by Prof. E. N. da C. Andrade, Lord Rayleigh and Sir James Jeans at the Royal Society's commemorative meeting. In the words of Sir Henry Dale, in opening that meeting, Prof. Andrade was asked to survey "the change which Newton's work produced, in the conceptions of the material universe which were current in his own times". Lord Rayleigh was asked "to deal, by demonstration where possible, with the experimental work of Newton and the great discoveries which he made by that method". Finally, Sir James Jeans was asked to give "some reassessment of the validity and permanence of Newton's system, in relation to the immense advances of knowledge in our own times".

In these four contributions, we believe we have a conspectus, brief as it must necessarily be, of the life and work of Newton. For more detailed accounts of the several aspects of Newton's work we must refer readers to the Supplement published with NATURE of March 26, 1927, on the two hundredth anniversary of his death. That issue included also a representative collection of portraits, and photographs of the well-known statue in Trinity College Chapel, Cambridge, and the Manor House at Woolsthorpe.

On p. 731 of this issue we reproduce an interesting document belonging to the Royal Society, which bears the signatures of both Newton and Halley. It was among the exhibits arranged by the Royal Society for the celebrations on November 30 last.

Newton's name will ever be remembered as the author of the "Philosophiæ Naturalis Principia Mathematica". Nevertheless, it is gratifying to know that, as the result of steps taken by the Royal Society, and through the generosity of the Pilgrim Trust, the Manor House at Woolsthorpe has been secured for posterity as a memorial. A suggestion made by Dr. H. Buckley in a lecture to the Institute of Physics (see p. 731), for a complete edition of Newton's works, is well worthy of consideration. As he pointed out, magnificent editions have been published at the expense of their countries of the works of Galileo, Huygens and Fresnel ; Britain has no such memorial of one who is rightly claimed as the most distinguished of natural philosophers.

NEWTON: SCIENTIST AND MAN

By PROF. S. BRODETSKY

ISAAC NEWTON was born on Christmas Day, 1642; he died on March 20, 1727. His life extended over one of the most significant and exciting periods in British history. When he attended the small village schools near Woolsthorpe in south-west Lincolnshire, Parliament gained its victory over King Charles I. When, as a boy of fifteen, he disappointed his mother's desire that he should administer the modest paternal farm, Cromwell died. When he returned to the Grantham School in order to prepare to go to Cambridge, and was passing through what was his only and unsuccessful romantic episode, the merry monarch King Charles II was restored. The Royal Society was founded when he was a young Fellow of Trinity. When at the age of forty-four his "Principia" was going through the press, he represented the University of Cambridge in its struggle against King James II. He refused to entertain any question of compromise, and approved of the revolution of 1688, for he represented the University as a Whig in the Convention Parliament of 1691. Although he took no part in the debates, for he was a very indifferent speaker and easily embarrassed when expected to make any statement unprepared, he played a considerable part in adjusting the relations between his University and King William III and Queen Mary. It was in the days of Queen Anne that he became the acknowledged representative of English science and received a knighthood. His death in 1727 was followed by the death of King George I, the first of that series of monarchs under whom British democracy was established.

From his graduation in 1665 until March 1696—a period of thirty-one years—Newton devoted himself to a career of scientific discovery which has not been equalled by any other human being. In 1696 he went to London at the invitation of his young Cambridge friend, Charles Montague, who had become Chancellor of the Exchequer, in order to act as Warden of the Mint, and for another thirty-one years he lived in the midst of national affairs. There has been a theory that between his great academic career and his London life Newton had gone through a serious illness involving mental derangement. There is no evidence that his mind was ever affected, although for a time he needed very careful attention.

During the second part of his life Newton retained his full mental vigour, and often carried out difficult mathematical investigations with great success. But it seems that he had lost interest in scientific discovery as such. There is no doubt that the services that Newton rendered to the nation in helping to improve the currency were of considerable importance; but having done this with success and achieved the reward of being appointed Master of the Mint—a semi-gentlemanly position with a large salary and small responsibility—he resigned his chair at the University of Cambridge and was very happy to be the doyen of British science for the remainder of his life.

As a young man Newton possessed two great passions—the love of calculation and the love of experiment. He evidently considered himself to be an experimentalist rather than a theoretical thinker. He devoted very long periods of time to laborious and complicated chemical experiments which led to no serious results. The fact that the man who established the mechanical method on which science has been based for two-and-a-half centuries was so intensely interested in the work of the alchemists of the Middle Ages, and devoted so much attention to the problem of converting base metals into gold, is an interesting illustration of the transition from the science of the Middle Ages to the new era inaugurated by himself: an era of unparalleled triumph in the mechanical interpretation of Nature, the philosophic foundations of which are only being questioned in our own day through the work of Einstein and the new quantum theory, based upon phenomena in the very large and the very small, far beyond any experimental evidence available until more than two centuries after the "Principia" was written.

Newton's aim seems to have been to explain natural phenomena in terms of forces acting on matter, and on the whole he refrained from framing explanations concerning the matter and the forces that he assumed in his work. His famous statement *hypotheses non fingo* was used when he defended himself against Leibniz's accusation that in his law of gravitation he had introduced occult methods into science and taught views contrary to a true belief in God, and really amounted to saying that his task was to explain under what laws things happened in Nature, and that he did not try to deal with the ultimate issues of the universe.

Newton's scientific work consisted of three parts: the optical, the mathematical, and the astronomical. It is scarcely necessary to explain what a remarkable achievement this work represented. It is sufficient to summarize the contents of the "Principia". This is not a text-book of existing knowledge with original additions here and there: it is one of the most original books ever written. The Galileo–Newton laws of mechanics are here formulated for the first time; the mathematical ideas required in the arguments, equivalent to the calculus, without which modern mathematics is unthinkable, are those invented by the author himself; the force of universal gravitation, invoked to explain the heavenly motions, is postulated as the result of Newton's own intuition; the study of fluid motions is initiated in order to dispose of the Cartesian hypothesis of heavenly vortices—the prevalent view held by all scientific men then; the phenomena explained in the "Principia" had never been in any sense satisfactorily explained before; the problems initiated, like lunar theory, planetary theory, shapes of planets, comets, tides, precession, etc., have occupied the best minds of humanity for centuries; the results achieved have been the admiration and inspiration of hundreds of mathematicians and astronomers, and will continue to be so for many centuries to come—and with it all there is a spirit of simplicity, dignity, sanity and soberness, and a grandeur of conception, that place Newton and his work on an unequalled level of eminence.

We are now living in a time when youth is "coming into its own"; often it is misled into hopeless misery. But youth is a characteristic of scientific progress. Einstein once said: "A person who has not made his great contribution to science before the age of thirty will never do so". This was abundantly true of Newton. His three contributions to knowledge—the spectrum, fluxions, and gravitation—were developing when he was a young man of twenty-three. The plague of 1665 drove Newton to his Lincolnshire home, and for a year or two he was more or less cut off from scientific contacts; but many notes made then testify to the growth of these ideas in his mind.

It is unimaginable that one person could have done all that Newton achieved, working alone without help or co-operation from others, interrupting his work by brief and no doubt almost unintelligible lectures, getting his recreation by walking in the little garden that was attached to his rooms at Trinity College, and often spending weeks on futile chemical experiments searching for something in the constitution of matter which he never understood, and which was quite beyond the understanding of those days. Few mathematicians or astronomers have read the "Principia": it is a unique experience to follow Newton's awe-inspiring genius in the original. It is a similar experience to read his "Opticks", and to think that the man who invented these remarkable experiments, leading on by unimpeachable logic to that which laid the foundation of so much of the physics, chemistry, astronomy and technology of the nineteenth and twentieth centuries, was at the same time almost in secrecy evolving new and revolutionary mathematical methods on which he based the "Principia".

Newton had done his work by the time he was forty-four. It is true that he published his "Opticks" when he was sixty-one, but the book largely represented his youthful work. He published later editions of the "Principia", and he made efforts from time to time to deal with lunar theory, but later workers built up systems, planetary and lunar, of which he dreamt. One can easily conceive the whole of Newton's scientific work after the "Principia" eliminated, without in any way affecting the place that he occupies in scientific history.

Newton spent much energy on non-scientific problems. He refused to take Holy Orders in order to retain his fellowship at Trinity, and got special permission from King Charles II to retain his fellowship while remaining a layman. This was not due to light caprice or indifference. Newton was deeply interested in theological philosophy. He had a thorough knowledge of the Bible; he recorded his views about God and Christ, and even attempted to publish—outside England—statements which implied doubts concerning fundamentals of the accepted faith. He was inclined to mystical views. But he was an anti-Catholic and not quite clear about the nature of miracles. He made a very clear distinction between human rights and divine sanction. It was Newton who wrote: "The authority of emperors, kings and princes, is human; the authority of councils, synods, bishops and presbyters, is human; the authority of the prophets is divine". He fought against royal attempts to exercise unlimited authority. He went to Parliament on two occasions as a confirmed Whig, and was a keen opponent of anything which gave to human beings powers that could only be attributed to the divine. It is difficult to compare political views in different periods, but it is not incorrect to suggest that Newton was what we would call to-day an advanced Liberal.

There is a fine streak in Newton's character, showing the immortal scientist behaving with the greatest care and kindness to young men in whom he saw signs of ability or genius. He was especially friendly with Halley, no doubt in gratitude for the way in which Halley carried the financial as well as the editorial responsibilities in the publication of the "Principia"; he was kind and appreciative of Roger Cotes, Colin Maclaurin, James Stirling; he supported the peculiar William Whiston. Newton was very generous even when he was not wealthy. He remained the owner of his small hereditary estate in Lincolnshire, and some of his letters indicate his intense interest in such minor matters as the inhabitability of cottages and the comfort of his tenants.

But Newton's life was spoilt by a series of quarrels with some of the most brilliant men of his time. Even as a young man he could not stand criticism of his scientific work, and announced that he would not publish anything at all if he had to be a "slave to philosophy" and spend his time answering criticisms. It seems that this was why he became an open enemy of that erratic genius Robert Hooke. John Locke described him as "a nice man to deal with", with the Shakespearian meaning of "nice". He had persistent quarrels with John Flamsteed, that brilliant, poor and modest first Astronomer Royal, who had to earn his living as a vicar as well as by private teaching, and who helped Newton with information for the "Principia" and for his efforts on lunar theory. Newton seems to have been a rather unworthy participant in the great controversy with Leibniz and John Bernoulli concerning priority in the discovery of the method of the calculus. These quarrels throw a peculiar light on his personal and scientific character, unworthy of a great thinker. He even wanted to leave the astronomical part of the "Principia" unwritten in case it was subjected to criticism. It seems that in his Cambridge life Newton wanted to be free from all discussion, so as to work privately and uninterruptedly on some things that were useless, and on some which form the glory of human discovery. When he was the public man in London he seemed to possess an extraordinary belief in his own achievements, and treated some other people in a way unworthy of a leader of science.

Newton made some serious mistakes in his scientific work. His optical ideas held up the development of the achromatic telescope, and were an obstacle to the evolution of the wave theory of light. It seems that his primitive theories on fluid resistance kept some people in our own generation dubious of the possibility of flight. Would he himself have disapproved of slavish subservience to his own authority?

One may be doubtful of this, but such was Isaac Newton, endowed with unimaginable mental qualities and with obvious faults; the down-at-heel professional bachelor of his academic days, and the rather wealthy Court favourite of his later years; the man who believed in freedom, who fought the tyranny of a king, and in his own way helped to develop British democracy. Newton was not a statesman or a politician. He was not technically a philosopher. He never recorded any social theory. But it is well to-day to remember him not only as the man who made the heavens intelligible to humanity, but also as a man who helped to give liberty to humanity so that it may be free to look up into heaven.

NEWTON AND THE SCIENCE OF HIS AGE

By PROF. E. N. DA C. ANDRADE, F.R.S.

IT is my task—my honourable and inspiring task—to say something of Isaac Newton as seen against the background of the science of his time. I shall try to display briefly the position as he found it and to resume in a small space his great achievements and the changes in outlook which they produced. In praising Newton I shall endeavour to do justice to his great forerunners and to the men of his time who pursued worthily the same great ends as he did, and who would have held the centre of the stage in any other age than that dominated by him. For Newton, like Shakespeare, did not stand as a lonely adventurer into new realms, though he travelled farther and straighter than the rest. Shakespeare was the supreme poet and playwright at a time when poetry and plays were part of the life of every cultivated man and occupied the attention of the brightest intellects. Newton was the supreme scientist in an age when the quantitative method of questioning Nature was abroad in the air. Each was the child of his time.

Let us consider the position when Newton went to Cambridge in 1661. The hold of Aristotle, whose works had for centuries been the ultimate resort of all those seeking knowledge of the working of Nature, had been shaken off by such men as Galileo and Gilbert, but most of the learned still thought that those who relied on experiment were pursuing a futile and impudent course. The first resolve of Marlowe's Faustus

> "Having commenc'd, be a divine in shew,
> Yet level at the end of every art,
> And live and die in Aristotle's works"

still represented the aim of many students. The foundation of the Royal Society in 1662 had been the occasion of many attacks on the experimental method, attacks stoutly met by Glanvill and Sprat, and as late as 1692 Sir William Temple's "Essay upon the Ancient and Modern Learning", satirized by Swift in "The Battle of the Books", set out to prove the superiority of the philosophers of the ancient world over all the moderns. Thus when Newton was a young man the new experimental method of questioning Nature was steadily making its way and the omniscience of the ancients was being called in doubt by a new school, but experimental science was by no means firmly established as a respectable study.

The great figures among the worthies of the exact sciences who had already appeared at that time were Copernicus, Tycho Brahe, Kepler, Gilbert, Galileo and Descartes. Kepler, following his great forerunners, had found the true laws of planetary motion, which were to be explained by Newton. Kepler's views as to the mechanism of the planetary motions were in his earlier writings largely mystical, involving the perfect properties of the five regular solids and also certain motivating souls or spirits. Throughout he held to the medieval point of view that a body could not maintain its motion unless there were a force propelling it. In his later writing he invoked a magnetic force, but it was not directed to the sun, like the true gravitational force, but pushed the planets on their way—"non est attractoria sed promotoria". It was essentially bound up with the rotation of the sun. Thus he was ignorant of the basic laws of mechanics, and his magnetic force had none of the true properties of magnetic forces. Kepler made no approach to a mechanical explanation of his laws. Gilbert had not only established the basic principles of terrestrial magnetism and carried out fundamental work on electricity, but also had invoked a force from the moon—a magnetic force it is true—to produce the tides. Galileo's greatest achievement had been to lay the foundations of mechanics.

None of these men, however, had made any impression on the bulk of the learned: Francis Bacon, for example, does not mention Galileo or Kepler, and refuses to take Gilbert seriously. The great figure in the eye of natural philosophers was Descartes, who had developed a cosmogony based upon mechanical principles, not precise mechanical principles it is true, but principles very different from the mystical ones then in vogue. Joseph Glanvill used to lament that his friends had not sent him to Cambridge, where he might have learned the new philosophy of Descartes, rather than to Oxford, where Aristotelianism ruled. Descartes' system had acquired such a hold on men's minds that his views were still supported long after Newton's death. Both Jean Bernoulli, who died in 1748, and Fontenelle, who died in 1757, were Cartesians to the end. The demolition of the Cartesian system was, in contemporary eyes, one of Newton's greatest achievements. James Thomson said, in his "Ode to the Memory of Sir Isaac Newton", published immediately after his death:

> "The heavens are all his own; from the wild rule
> Of whirling vortices, and circling spheres,
> To their first great simplicity restored.
> The schools astonished stood."

From the "Principia" it is clear that Newton himself derived particular satisfaction from having invalidated the Cartesian system. It is fitting, then, that we start our consideration of contemporary science by a glance at this system, not only because it was the only attempt before Newton to explain the motions of heavenly bodies on general principles, but also because it furnishes a contrast which brings out the essential Newtonian point of view.

Descartes starts, in the spirit of medieval thinkers, from certain general philosophical principles. He decides that the fundamental property of matter is extension—impenetrability, colour, hardness and so on are only secondary characteristics. Extension, which has three directions, is the subject of mathematics: motion is the subject of mechanics. All the different qualities of different kinds of matter are provided by different motions of the minute parts of which they are composed. "Give me extension and motion," declares Descartes, "and I will construct the world." One consequence of his fundamental belief is that there cannot be a vacuum, for extension without matter is a contradiction. He further considers, on theological grounds, that the quantity of motion in the heavens must be constant. He blames Galileo for founding his mechanics on experiments and not on reflexions on first causes. "Every-

thing Galileo says about the philosophy of bodies falling in empty space is built without foundation: he ought first to have determined the nature of weight." Newton's point of view, of course, was the exact opposite to that of Descartes: he says in the famous letters to Bentley, ". . . for the cause of gravity is what I do not pretend to know, and therefore would take some time to consider of it"; and again, "gravity must be caused by an agent acting constantly according to certain laws, but whether this agent be material or immaterial, I have left to the consideration of my readers". For Newton, as for the best of his successors, science was concerned with the question of "How ?": Descartes, like the ancients, was concerned with the insoluble question of a fundamental "Why ?".

It followed from the philosophic hypothesis of Descartes that the only kind of motion possible in a plenum was a motion in closed paths, more particularly a circular motion, since a particle could only move if another particle took its place. It was on grounds of this kind that he elaborated his vortex hypothesis. Certain very fine particles, which filled interplanetary space, moved round ceaselessly in huge vortices and carried the planets with them. The moon was carried round the earth by a minor vortex, and so on.

Descartes' cosmogony, then, was founded on a philosophical system : it was pictorial and unquantitative. The paths of the comets handed on from one vortex to another were in particular irreconcilable with observation. There was no attempt to deduce Kepler's laws, or to show how anything but circular motion could result from the vortices. The whole scheme was spun from the brain of Descartes, with more or less casual references to actual phenomena. No doubt it was referring to Descartes that Roger Cotes said in his preface to the second edition of the "Principia", "Those who fetch from hypothesis the foundation on which they build their speculations may form indeed an ingenious romance, but a romance it will still be". In spite of, or perhaps because of, this, Descartes' influence was immense, and when Newton was at Cambridge as a young man it was Descartes who was the great authority for all such as speculated on the structure of the universe.

We now turn to the astonishing story of the birth of the "Principia". The time was ripe for the appearance of this great work. As regards the laws of motion, Galileo, whose services Newton freely acknowledges, and Descartes himself had done much to prepare the way for the more precise and particular formulation which Newton gives. Hooke among others had clearly expressed the protest of the most forward spirits of the time against the speculative method. "The truth is, the Science of Nature has been already too long made only a work of the Brain and the Fancy: it is now high time that it should return to the plainness and soundness of *Observations* on *material* and *obvious* things." The Royal Society was active in stimulating the pursuit of the new method, and on the Continent the Accademia del Cimento had done excellent work, while the Académie des Sciences was founded in 1666. These Continental societies had little influence on Newton, but are symptomatic of the general movement towards the experimental method. The stage was set for great things.

The story opens at Woolsthorpe, Newton's birthplace, whither Newton had come from Cambridge in June 1665 to escape the plague. He was then twenty-two years old and not yet a master of arts or fellow of Trinity. He had read what he calls "Schooten's *Miscellanies*" (probably the "Exercitationum Mathematicarum Libri V"), Descartes' "Geometry" and Wallis' works, and further was, of course, familiar with the work of his teacher Barrow. He had written his first treatise on the calculus, or 'fluxions' as he called it, but he had published nothing. The words which he wrote some fifty years later about this great springtime of his intellectual life have often been quoted but cannot well be omitted on an occasion like this. "In the same year (1666) I began to think of gravity extending to the orb of the moon, and having found out how to estimate the force with which a globe revolving within a sphere presses the surface of the sphere, from Kepler's Rule of the periodical times of the planets being in a sesquialterate proportion of their distance from the centre of their orbs I deduced that the forces which keep the planets in their orbs must [be] reciprocally as the squares of their distances from the centres about which they revolve : and thereby compared the force requisite to keep the moon in her orb with the force of gravity at the surface of the earth, and found them answer pretty near. All this was in the two plague years of 1665 and 1666, for in those days I was in the prime of my age for invention, and minded mathematics and philosophy more than at any time since. What Mr. Huygens has published since about centrifugal forces I suppose he had before me."

It seems likely that he had already his laws of motion in his head—in any case it is clear that he was convinced that every body would continue to move uniformly in a straight line unless some force acted on it, and that, therefore, there must be some force acting on the moon which drew it away from the straight line, tangential at any moment to its path, in the direction of the earth. Treating the moon's path as circular, from Kepler's third law, and from the law connecting the centrifugal force with the radius and the velocity, or the equivalent proposition to which Newton refers, it is easy to deduce the inverse square law. To show that the force keeping the moon in her orbit is the earth's gravitational force, assumed to diminish as the inverse square, is, however, a further step demanding a computation of how strong the gravitational force at the moon's orbit will be, compared to the measured force at the surface of the earth. Newton made this step and found it "answer pretty near". Why, then, did he delay the announcement of the law of gravity for twenty years or so ?

There is always a ready answer to questions of this kind where Newton is concerned—that he never published anything until invited, in general strongly urged, to do so. It is, however, clear from many signs that Newton was not himself satisfied about the matter until some time about 1685. The usual story is that he took a wrong radius for the earth, namely one corresponding to 60 miles for 1° of latitude instead of the correct value of about 70, but this story is very improbable on many grounds, one of which is that good values were readily available to him.

The real reason for Newton putting the work aside seems to have been that the calculation, so far as the force at the earth's surface is concerned, depends essentially upon it being legitimate to assume that the earth's mass may be considered as concentrated at the centre. That this assumption is valid is far from obvious. It is fairly clear from certain passages in "De Motu" and in the "Principia" that it gave Newton some trouble to prove this assumption and

that he did it late. In a letter to Halley of June 20, 1686, he says, "I never extended the duplicate proportion lower than to the superficies of the earth, and before a certain demonstration I found the last year, have suspected that it did not reach accurately enough down so low". Although this refers to the gravitational force within a sphere, this and the point under discussion are involved in the same mathematical demonstration, which Newton gives in the "Principia" in Book 1, Proposition LXXI and other propositions following it. In any case Newton appears not to have been satisfied with his first calculations and to have turned to other things, possibly his optical experiments.

It is a strange thing that the "Principia" owes its publication largely to a quarrel with Hooke, and its sequel. In 1679 Hooke, then acting as secretary of the Royal Society, wrote to Newton about various scientific matters and asked him very civilly for a philosophic communication—a paper as we should say nowadays. He also asked for Newton's opinion on his "Potentia Restitutiva" and "particularly if you will let me know your thoughts of that of compounding the celestial motions of the planets of a direct motion by the tangent and an attractive motion towards the central body". In his reply Newton made an extraordinary remark. "But yet my affection to philosophy being worn out, so that I am almost as little concerned about it as one tradesman uses to be about another man's trade or a countryman about learning, I must acknowledge myself averse from spending that time in writing about it which I think I can spend otherwise more to my own content and the good of others: and I hope neither you nor anybody else will blame for this averseness." This is but one of many occasions on which Newton expresses his disinclination, almost distaste, for any further scientific work, his first antipathy having been aroused by the disputes and misunderstandings consequent on the publication of his first great work on the prism.

Nevertheless, Newton did comply with Hooke's request for something for the Society by pointing out that a body let fall from on high should strike the earth slightly east of the perpendicular, and gives precise and excellent directions for following out the experiment. The purpose was to prove the diurnal rotation of the earth. Hooke, in reply, pointed out that the ball should fall to the south as well as to the east, and further corrected Newton in a point which is too complicated to discuss here and one on which differences of statement can be due to different interpretations of the problem. This correction, tactlessly expressed, irritated Newton in the highest degree and he answered curtly. In further letters, written in apparent unconsciousness of the annoyance he had given, Hooke suggested quite explicitly that the law needed to explain the planetary motions was the inverse square law.

Now other men had come to the same conclusion. In particular, Wren and Halley had discussed with Hooke the possibility of explaining the mechanism of the heavens on the basis of an inverse square law. Hooke declared that he could demonstrate mathematically that the path of a particle in a central inverse square field would be an ellipse, but it is clear that he never did so, and equally clear that he had not the mathematical equipment necessary to begin an attack on the problem. He was in the unfortunate position of being entirely convinced of a truth that he could not prove. In August 1684 Halley visited Cambridge and asked Newton what the path would be. He replied that it would be an ellipse and that he had formerly calculated it. He could not find the calculation but soon sent a proof (or apparently two different proofs) to Halley.

This incident seems to have aroused Newton from the distaste for science into which he had fallen, and he put together the treatise "De Motu", founded on a course of lectures, which Halley presented to the Royal Society on December 10, 1684. The story of how Halley then coaxed and cajoled Newton into writing the "Principia" is familiar, but we in the Royal Society ought never to celebrate the great work without a tribute to Halley, who not only realized at once the fundamental importance and significance of Newton's work but also used all his tact to get the book written and made himself financially responsible for the production, the Royal Society being in financial difficulties at that time. We are not now in financial straits and shall, I believe, be glad to bear the expense of producing a second "Principia" when the genius of our age brings it forth. The book appeared in 1687, and bears the imprimatur of the then president, who, although he achieved nothing in science, is still remembered. He was Samuel Pepys.

The "Principia" is not an easy book to read. The proofs are all given in the form of classical geometry, although, since it is certain that at the time when it was written Newton was in possession of the fundamental processes of the calculus and of the methods of analytical geometry, it is unlikely that this was the form in which he first derived them. Whewell has said, "Nobody since Newton has been able to use geometrical methods to the same extent for the like purposes; and as we read the 'Principia' we feel as when we are in an ancient armoury where the weapons are of gigantic size; and as we look at them we marvel what manner of man he was who could use as a weapon what we can scarcely lift as a burden". Various conjectures have been made by Rosenberger, Cantor, Giesel, Gerhardt and others as to why he did not use the new methods: Rouse Ball thinks that he was probably unwilling to add to the difficulties by introducing a new mathematical method. Be that as it may, he had a horror of unfounded criticism, and, as he told Dr. Derham, "to avoid being baited by little smatterers in mathematics, he designedly made his 'Principia' abstruse; but yet so as to be understood by able mathematicians". That he did not underrate the difficulty of the work is clear from what he says in the beginning of Book III: "I chose to reduce the substance of that book into the form of propositions (in the mathematical way) which should be read by those only, who had first made themselves masters of the principles establish'd in the preceding books. Nor would I advise any one to the previous study of every proposition of those books. For they abound with such as might cost too much time, even to readers of good mathematical learning. It is enough if one carefully reads the definitions, the laws of motion, and the first three sections of the first book."

The first book contains certain definitions of space, mass and time which have afforded the more metaphysically inclined a theme ample enough for discussion. But we will here let them pass uncommented. The famous laws of motion owe much to the labours of previous workers, in particular to Galileo, whose services Newton clearly acknowledges. The simple laws of central orbits under an inverse square law are worked out in much detail and the laws of

pendulum motion are developed, with due acknowledgments to Huygens, who had published his "Horologium Oscillatorium" in 1673.

In the first book the motions are all supposed to take place in a non-resistant medium. In the second Newton considers motions in a resisting medium, always with his eye on the Cartesian world system which he was intent to demolish. He deals with a resistance proportional to the velocity and a resistance proportional to the square of the velocity, and further points out different kinds of fluid resistance, which he later defines most clearly in the 28th Query appended to the third edition of the "Opticks", "for the resisting Power of the fluid Medium arises partly from the Attrition of the Parts of the Medium and partly from the *Vis inertiae* of the Matter", that is, partly from the viscosity and partly from the bulk motion of the medium. The latter resistance he assumes proportional to the square of the velocity. In this book he opens the way to the hydrodynamics of real fluids. In the part dealing with hydrostatics he proves the law of the diminution of atmospheric pressure with height. He then discusses the motion of the pendulum and is the first to suggest its use for making a survey of the gravitational acceleration. In another connexion he derives an expression for the velocity of sound, this being the first case of a calculation of the velocity of a wave from the properties of the medium. The only other point in this book to which I will refer is the calculation which Newton carries out on the motion of an infinitely extended viscous fluid in which a body rotating upon its axis is immersed. What he has in mind is the Cartesian vortex—"I have endeavoured in this proposition to investigate the properties of vortices, that I may find whether a celestial phenomenon can be explained by them". He effectively defined the force "arising from the want of lubricity in the parts of the fluid" as proportional to the velocity gradient, whence the term 'Newtonian viscosity'. He is thus the first to touch the mathematics of viscous fluids. He finds that the periodic time of circulation of the fluid carried round by the rotating sphere is proportional to the square of the distance from the centre of the sphere*, which is grossly inconsistent with Kepler's third law. Newton considers that his deduction is a clear refutation of the Cartesian vortices and, for once, almost gloats over his victory. "Let philosophers then see how the phenomenon of the sesquialterate ratio can be accounted for by vortices". He brings many other objections against the Cartesian vortices: he points out, for example, that a continuous supply of energy will have to be given to the sphere to maintain the motion, because "it is plain that the motion is proportionally transferred from the centre to the circumference of the vortex, till it is quite swallowed up and lost in the boundless extent of that circumference".

The second book of the "Principia" is the foundation stone of mathematical physics. In it Newton shows an extraordinary instinct for grasping the essentials of a problem—it would almost seem that he knew the solution in advance and added the proof as a concession to those less clear-sighted. The manner of his refutation of the Cartesian vortices introduces a new spirit into the discussion: no hypothesis about the heavens is tenable unless the quantitative deductions from it agree with observations.

The third book opens with an introduction where, after stating what has been done in the first two books, Newton sets down the superb sentence, "Superstat ut ex iisdem principiis doceamus constitutionem systematis mundani"—it remains that from the same principles we demonstrate the form of the system of the world. It is this third book which based celestial mechanics so firmly that what was done in the next two hundred years was rather extension of, and improvements on, the Newtonian method than anything radically new. Not only does Newton establish the movements of the satellites of Jupiter, Saturn and the earth, and of the planets round the sun (or rather, as he points out, round the centre of gravity of the solar system) in terms of his gravitational theory, but also he shows how to find the masses of the sun and planets in terms of the earth's mass, which he estimates quite closely to the present accepted value; he accounts for the flattened shape of the earth and other planets; calculates the general variations of g over the surface of the earth; explains the precession of the equinoxes by consideration of the non-sphericity of the earth; calculates the main irregularities of the motion of the moon and of other satellites from the perturbing effect of the sun; explains the general features of the tides; and finally treats the orbits of comets in a way that showed that they were members of the solar system and enabled the return of Halley's comet in 1759 to be accurately calculated. This brief and imperfect catalogue is merely a reminder of the scope of this extraordinary book, which drew from Laplace, no enthusiast, ". . . all this, presented with much elegance, assures to the 'Principia' preeminence over all the other productions of the human mind". The book closes with the famous general scholium which returns to the confutation of Descartes' vortices and says, concerning the cause of gravity, "Hypotheses non fingo".

Although the book was eagerly bought, the Newtonian method and discoveries made way but slowly. Biot says that of Newton's contemporaries three or four only were capable of understanding the "Principia", that Huygens only half adopted the ideas, Leibniz and Jean Bernoulli fought against them, and that fifty years had to pass before the great truth demonstrated by Newton was understood by the generality of men of science, let alone developed. No doubt the difficulty of the book had much to do with the tardy appreciation, outside a narrow circle, of its contents. The Cartesian scheme was easy, pictorial, general: the Newtonian difficult, mathematical, precise. The very method of attacking the problem was altogether new. The second edition appeared in 1713, edited by Roger Cotes, of whom Newton said, "if Mr. Cotes had lived we might have known something". The preface clearly shows that among the learned the Newtonian scheme had not been widely accepted. On the Continent the progress was still slower and it is generally held that it was Voltaire's "Elémens de la Philosophie de Neuton", which appeared in 1738, that led to Newton's work being appreciated in France. Incidentally, the story of the apple, which Voltaire had from Newton's niece, Mrs. Conduitt, first appeared in the second edition of Voltaire's book, in 1741. Later, it was in France that Newton's work was raised to great glory, when Lagrange and Laplace erected edifices of splendour and elegance on Newtonian foundations.

* Actually this is an error, which appears never to have been pointed out, although Stokes has indicated the like error in the case of the rotating cylinder, which Newton works out. It should be the *cube* of the distance.

The optical work of Newton was perhaps his favourite study: at any rate it was that to which he gave what is probably the only enthusiastic reference which he ever permitted himself, when he said, of his fundamental discovery in the matter of prismatic colours, "being in my judgment the oddest, if not the most considerable detection which hath hitherto been made in the operations of nature". I do not propose to say anything of the beautiful experiments which form the backbone of the book, but a few words as to the Newtonian attitude to the theoretical aspect may be permitted.

The nature of colour had from the days of Aristotle been the subject of philosophical speculation. According to Aristotle, colours are a mixture of light and darkness, or of white and black, a view which, embellished and modified in various ways, survived Newton and appeared again in, for example, Goethe's writings. Descartes was apparently the first to break really new ground in comparing colours to notes in music: his view of light was that it was a pressure transmitted through the particles that filled all space, and he conjectured that a rotation of the particles might be the effective cause of colour, a view which presents inherent difficulties effectively exposed by Hooke. We may agree with Huygens that "Descartes has said nothing that is not full of difficulties or even inconceivable, in dealing with light and its properties". The medieval question as to whether light was a substance or an accident was still occupying the attention of even acute experimentalists such as Grimaldi, the discoverer of diffraction, whose book appeared in 1665. It was Hooke again, who, with his extraordinary flair for the truth, combined with an inability to overcome the last difficulties that stood between him and a convincing conclusion, initiated the modern views. In his "Micrographia", 1665, he expressed the view that light was a very quick vibration propagated with a finite velocity. He gave what is something like Huygens' construction for finding the wave front on refraction, but, since he thought that light travelled faster in a solid medium, such as glass, than in air, he found that in the medium the wave front must make an acute angle with the ray. It was with this 'obliquity' that he connected colour. "Blue is an impression on the Retina of an oblique and confus'd pulse of light, whose weakest part precedes, and whose strongest follows. . . . Red is an impression on the Retina of an oblique and confus'd pulse of light, whose strongest part precedes and whose weakest follows." For him blue and red were the primary colours, all others being mixed. There is no time to follow his extraordinarily acute experiments on the colours of thin plates, but it must be noted that his ill-tempered attacks were the cause of Newton expressing a disgust with science which nearly caused him to abandon her pursuit, and were, it is almost certain, the reason why the "Opticks" was not published until 1704, the year following Hooke's death. The quarrel between the two men, both, as is evident from their correspondence, capable of generous appreciation of the other's achievements, was exacerbated by Oldenberg, then secretary of the Royal Society, whose dislike of Hooke may have sprung from causes little creditable to him. Newton had been undoubtedly stimulated by his reading of Hooke's "Micrographia", and he was always very remiss in acknowledgments to him. Let us pay a tribute to poor Hooke, sickly and without position or powerful friends. He had not Newton's power of thought, but he was probably the most ingenious contriver who ever lived and was a shrewd and daring speculator.

Nowhere more than in his writings on light does Newton stress his dislike of speculation not firmly rooted in experiment. The first words of the "Opticks" are "My design in this Book is not to explain the Properties of Light by Hypotheses, but to propose and prove them by Reason and Experiments", and again, in the 31st Query appended to the third edition of that book, he makes his point of view very clear, saying "These Principles I consider not as occult Qualities, supposed to result from the specifick Forms of Things, but as general Laws of Nature, by which the Things themselves are form'd: their Truth appearing to us by Phaenomena, though their Causes be not yet discover'd. For these are manifest Qualities, and their Causes only are occult. And the *Aristotelians* gave the Name of occult Qualities not to manifest Qualities, but to such Qualities only as they supposed to lie hid in Bodies, and to be the unknown Causes of manifest Effects. . . . To tell us that every Species of Things is endow'd with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing." It is in the light of this that we must read the famous "Hypotheses non fingo". Newton, of course, did make hypotheses, and even called them such—for example, in "Principia", Book II, Section IX, his assumption as to the behaviour of viscous fluids is headed "Hypothesis", and there are other instances. All he meant was that he was reluctant to speculate beyond any possibility of quantitative deduction, to form conjectures the defence of which would be merely a matter of dialectics.

Newton's corpuscular hypothesis to account for the experimental behaviour of light is clearly a hypothesis, but we shall see how closely he adapts it to the observations. First, he points out that light cannot be a wave motion, or it would spread out on passing through an opening. He knew, it is true, the phenomena of diffraction, but he did not realize how by making the wave-length small enough this could be reconciled with the general facts of rectilinear propagation, for which a stream of particles seems best suited. He clearly saw that the phenomena of the colours of thin plates demanded a periodicity, and he introduced this into his theory by the hypothesis of fits of easy reflexion and easy transmission. It is clear that light is partly reflected and partly transmitted at the surface of a transparent body: Newton supposed that a light particle alternated at regular intervals between a state in which it was transmitted through the surface and one in which it was sent back. He put forward diffidently ("Those that are averse to assenting to any new Discoveries but such as they can explain by an Hypothesis, may for the present suppose . . .") the idea that the impact of the particle on the surface excited vibrations in the medium which, overtaking the particles, put them into these alternating states. We are forcibly reminded of modern theories. The length of the interval of the fit, corresponding to our wave-length, was greater for the red than for the blue, and Newton gives the interval for yellow light incident normally as just about the actual wave-length of yellow light. Unfortunately, the length of the fit depended on the angle of incidence. However, of his whole theory he says, "But whether this hypothesis be true or false I do not here con-

sider. I content myself with the bare Discovery that the Rays of Light are by some cause or other alternately disposed to be reflected or refracted for many vicissitudes". When he comes to consider polarization, he has to endow his particles with 'sides', so that there was a lack of complete symmetry about the direction of propagation. In short, he gave his particles just those properties which interpreted the experiments and hence was led to endow them with a periodicity and a polarity. This brought him to assume subsidiary waves accompanying the particles when they interacted with matter.

There seems to be a general belief that Huygens, as against Newton, advocated something very close to the wave theory of light, as it was accepted in, let us say, 1900. Huygens' wavelets, however, lacked the essential properties with which Fresnel afterwards endowed them: they were not only longitudinal but also had what are practically particle properties, as exemplified by the fact that the pole, the place where the wavelet touches the envelope, alone is efficacious. He was just as incapable as Newton of giving a satisfactory explanation of diffraction.

If we are asked to state in a sentence what was the main effect of Newton's work on the thought of his time, I think that the answer must be that it was to establish the power and universality of the methods of quantitative science. To Galileo we owe the great service, one that cannot be too highly praised, of having made particle dynamics into a science, but he did not look beyond the earth for its efficacy or suggest the application of his methods to the relative movement of the parts of a continuous medium, such as water. Huygens founded the study of rigid dynamics. Hooke suggested and speculated with extraordinary ingenuity and acuteness. Newton, however, showed that three clearly enunciated laws of motion applied to all observable movements of inanimate Nature: they governed the motion of waves and projectiles, visible solids and invisible air, resisted as well as free movements. Together with the inverse square law they explained not only the gross movement of planets and the movement of the comets, which before had seemed capricious, but also details which nobody had ever considered as being mechanically explicable, such as the precession of the equinoxes. The problems of the tides and of the irregularities of the moon's motion he did not fully solve, it is true, but he did enough to convince mathematicians that they were soluble by his methods. After Newton's work had been assimilated, the body of natural philosophers accepted it as a commonplace that all terrestrial and celestial movements were explicable in precise and numerical terms by calculations based on a few general laws: before Newton most thinkers were ready to invoke *ad hoc* principles and occult causes, based on human and divine analogies, for any but the simplest terrestrial phenomena, and the few who were in advance of their times were feeling tentatively for solutions which eluded their grasp.

Even in such thoughts on chemistry as he published Newton was looking for an explanation in terms of attractions, though, strangely enough, in this science he never seems to have applied his own rule and made quantitative experiments. His work on light lies somewhat outside the mechanical scheme, but here again his insistence on the quantitative created a completely new attitude towards colour. It became a subject for measurement and calculation, rather than one for discussion in terms of generalities.

If we are to try to represent Newton's achievements by some modern analogy, to construct some imaginary figure who should be to our times what Newton was to his, we must credit this synthetic representative with, I think, the whole of relativity up to, and somewhat farther than, the stage at present reached—we must suppose our modern Newton to have satisfactorily completed a unitary field theory. In light we must credit him both with having established the existence of spectral regularities and with their explanation in terms of the quantum theory. Possibly, too, we must give him the Rutherford atom model and its theoretical development, a simple astronomy in little to correspond to the solar system. Let us, then, think of one man who, starting in 1900, say, had done the fundamental work of Einstein, Planck, Bohr and Schrödinger, and much of that of Rutherford, Alfred Fowler and Paschen, say, by 1930, and had then become governor of the Bank of England, besides writing two books of Hibbert Lectures and spending much of his time on psychical research, to correspond with Newton's theological and mystical interests. Let such a man represent our modern Newton and think how we should regard him. Only so, I think, can we see Newton as he appeared to his contemporaries at the end of his life.

There are no discontinuities in Nature and there are none in the history of science. No discovery or fundamental innovation is absolutely new, unconnected with past thought and the stirring spirit of its own time. Newton was not uninfluenced by certain of his immediate predecessors and of his contemporaries. The revolt from the introspective method of constructing explanations of heavenly and earthly phenomena by appeals to philosophic necessity had begun before his birth, and his time was rich in brilliant exponents of the experimental philosophy, whose names will always stand as stars adorning the story of science. To compare him with other men of his time and to recognize their contributions to the development of the physical sciences does not, however, lead us to think less of Newton's achievements but rather to wonder at them all the more. It is easier to estimate the size of a Colossus if there are statues of more than life size in its neighbourhood than if it stand alone in a desert.

Newton owed much to the pioneer labours of Galileo, who had founded the science of mechanics, and, in a different way, much to Barrow, whose great mathematical acuteness and sympathetic support were always at the disposal of the young Cambridge scholar. To Hooke he owed more than he was ever prepared to acknowledge. From his great Continental contemporaries, Huygens and Leibniz, he borrowed little, if anything. Possibly if mathematical advances were alone in question Leibniz would have to be considered for a place on the same level as Newton, but for Newton mathematics were merely a means to a physical end: his mathematical innovations may even be left out of consideration without grave injury to Newton's fame. There is no record of physical experimentation that can compare for mastery and elegance with the "Opticks", no work in exact science that produces the same impression of supreme greatness and power of thought that the "Principia" does.

The spirit of this age is a denigrating one, which,

in a reaction from the earlier custom of representing great men as free from all blemish and weakness, delights in attempting to show that nobody much exceeds the common level. If an earlier painter might have left out Cromwell's warts, the painter of to-day might represent his face as one huge wart. I have read records of Newton's weaknesses and I know of the adumbrations of his discoveries that can be found in forerunners and contemporaries: I acknowledge that his earlier biographer shut his eyes to any incident, writing or action that might seem to detract from his perfection. Nevertheless, all things considered, I think that the contemporary judgment of his greatness can still stand, and that, if the Marquis de l'Hôpital's query as to whether Newton ate, drank or slept like ordinary men ("for I picture him to myself as a celestial genius") seems to our present-day sobriety an affectation, nevertheless we may agree that the line of Lucretius placed on the Trinity statue was well chosen and fitting—

Qui genus humanum ingenio superavit

"who excelled the human race in power of thought".

NEWTON AS AN EXPERIMENTER

By THE RIGHT HON. LORD RAYLEIGH, F.R.S.

THE duty has been assigned to me of telling you something about Newton as an experimentalist. As the result of a study of what is known of his history, it seems to me that among his various intellectual pursuits experiment was his first love and the love to which he was most constant. Strange though it be, he seems in some moods to have doubted whether his theoretical studies were worth while, and I do not recall any case where he expressed himself enthusiastically about them. On the other hand, he speaks of his optical work as "The oddest, if not the most considerable detection which hath hitherto been made in the operation of nature".

Newton loved the mechanical side of experimental work. As a boy he constructed sundials, and, what is more, fixed one of them into the side of the house effectually enough for it to be there a century later. A notebook of his boyhood shows him assiduous in collecting recipes for various kinds of drawing materials, and he notes methods of performing some (rather nasty) conjuring tricks. Later on, when he is making his reflecting telescope, it is obvious that he is a skilled amateur mechanic, at home in furnace operation. He builds his own brick furnace, prepares speculum metal, and is apparently more successful than the professional opticians of the time in grinding and polishing it to a satisfactory spherical figure. (The days of parabolizing were not yet.) It was not until a good many years later that they were able to put such instruments on the market.*

Asked in his old age where he got the tools for his work, he replied that he had made them himself, and could have achieved little progress without doing so.

There are occasional hints to be gleaned that Newton practised other mechanical arts. Thus, when he examines the colour of thin blown glass, it appears that he has the facilities for glass-blowing at hand, and was presumably able to use them.

So much for the base mechanical side. Newton, however, had what may be called the itch of experiment and instinctively examined in this way any natural phenomenon that excited his interest. This instinct is not a common one, and it would be of interest to investigate statistically whether it is more correlated with mathematical aptitude than with, say, an aptitude for literary and historical studies. Newton had all these. Although he experimented in other fields, such as mechanics, heat and electricity, and even in anatomy and physiology, his optical experiments are of much greater importance, and in the short time at our disposal we shall only be able to consider a part even of these. The fundamental researches on the composition of white light were read before the Royal Society in 1672, and afterwards recapitulated in his "Opticks" (1704), and we cannot do better than concentrate our attention on them. Although the results are common property nowadays, yet on an occasion like this we shall do well to go back to Newton's own methods and point of view, and to repeat his experiments as nearly as we can in his own way. We must be content to use the positive crater of the electric arc to represent the sun.

Newton's experiments on the spectrum are sometimes presented as if he had started out with the idea of examining the composition of white light. It is true that his "Opticks" (like his "Principia") introduces the various topics as theorems or problems proposed after the manner of Euclid. It does not seem likely, however, that he set out in the first instance to prove any proposition. He bought a prism at Stourbridge Fair (near Cambridge) in 1666, "to try therewith the celebrated phenomena of colours". It is clear from this that the prismatic colours were quite a well-recognized phenomenon at this time, and this is also shown by the circumstance that the art of cutting diamonds so as to display them was already long known. In Peacham's "Gentlemanly Exercises" (1612) reference is made to "A three square cristal prisme wherein you shall perceive the blew to be outmost next to the red", and Grimaldi and others had already experimented on the subject, though without arriving at clear views. We can readily imagine how Newton, handling the prism, would soon find that the colours were well seen in candle-light, but not in diffused daylight. It would not be a long step from this to try the effect on a beam of direct sunlight admitted through a hole in the shutter.

There is no reason to think that he did this with a very clear anticipation of what the effect would be. He was exploring a nearly virgin territory. His beam of sunlight passing through the hole produced an image of the sun. It was what we now call a pinhole image, though the hole need not be very small. Newton's hole was $\frac{1}{4}$ in. diameter. Then he interposed the prism (Fig. 1).

To most people the colours would seem the main

* Among the first successful commercial makers of reflecting telescopes was James Short (1710–1768), whose instruments were of the Gregorian type. He is said to have made a considerable number of concave and convex mirrors, and to have 'married' them by trial of what pairs gave the best result.

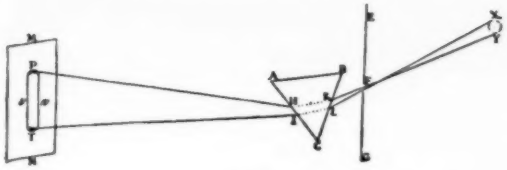

Fig. 1.

feature of this experiment, but Newton was more surprised and impressed with the fact that the sun's image was no longer round, but was spread out into a band of which the length was some five times the breadth. The breadth of the image as projected through the prism still answered to the sun's diameter, that is to say, it was equal to the diameter of the original round image of the sun at the same distance with the prism away.

"If," he says, "the refraction were done regularly according to one certain proportion of the sines of incidence and refraction, as is vulgarly supposed, the refracted image ought to have appeared round."

At the same time the colours had to be taken into account. Were the colours definitely related to the differences of refraction, or was the apparent relation only incidental? In Newton's words:

"Wherever this inequality arises, whether it be that some of the incident rays are refracted more, and others less, constantly or by chance, or that one and the same ray is by refraction disturbed, shattered, dilated and as it were split and spread into many diverging rays, as Grimaldi supposes, does not yet appear by these experiments, but will appear by those that follow."

The next experiment was accordingly designed to throw light on this point. It is known as the experiment of crossed spectra (Fig. 2), the idea being to refract each of the coloured rays a second time, in a direction at right angles to the former, and to see whether the blue rays, which were more refracted than the red, would be so again. Also to see whether they would retain their original colour, or whether they would undergo further analysis.

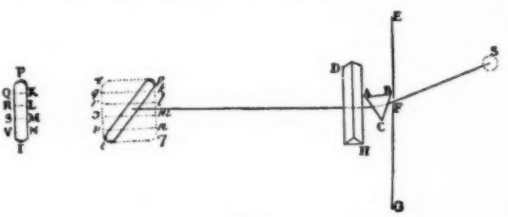

Fig. 2.

Newton made several more elaborate variants of this experiment. In one of them, two holes were used with a prism in front of each, and two spectra were then projected on the screen in line, the red of one following on the blue of the other. Refracting them upwards with a third prism, this line is dislocated, and that blue and red which were before adjacent are now widely separated (Fig. 3).

Newton in this way arrived at the view that the original white light of the sun consisted of coloured components, red, yellow, green, blue and violet. Each of these had a characteristic index of refraction and was bent to a perceptibly different extent by the prism. The result was that the original round white image of the sun was replaced by a series of coloured images side by side, overlapping and blending one with another. On introducing a second prism at right angles, each of these coloured images was again refracted to its characteristic extent and each retained its colour without further analysis.

Newton, however, soon saw the crudeness of his first arrangements, and we will now consider the methods he used to improve them. As he pointed out, the individual coloured images of the sun overlap at the middle, but not at the edges. Images of the sun are not in fact very suitable, because the angular width of the sun is about half a degree, and this is a considerable fraction of the angle between the red and violet rays. We want to make the sun in effect smaller, by blocking out part of its disk, which might be done in imagination by a distant diaphragm supported in mid-air. At the same time the hole in the shutter might with advantage be replaced by a lens, which would render the images sharper. When we have got so far in imagination, we notice that the long distance between the diaphragm in mid-air and the lens is not essential; with a suitable lens a moderate distance will do. We bring the diaphragm nearer, and fix it in the shutter, and bring the lens forward into the room, placing it so as to form a distinct picture of the diaphragm on the wall (Fig. 4).

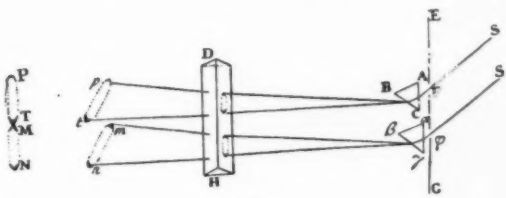

Fig. 3.

We have now the diaphragm fixed in the shutter, and backed by the sun, as our effective source of light. In this respect it differs altogether from the original diaphragm, which acted not as a source, but as an image-forming pinhole. We now have a picture of the diaphragm and not of the sun, and it will be advantageous to substitute a slit, which would not previously have been any use, because the size of the image was then defined by the angular diameter of the sun. With the slit *and the lens* we shall get narrow well-defined images in each colour, and the colours will be pure and not overlapping. (Needless to say, modern refinements carry the matter much further.)

The explanation I have just given is in substance Newton's own.

Let us now project the spectrum on to a small

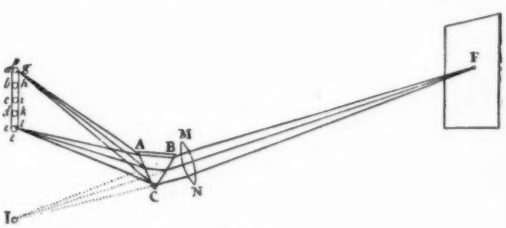

Fig. 4.

Fig. 5.

screen with a slit (Fig. 5) in it, which to avoid confusion I will call the screen-aperture. A colour passing through the screen-aperture is isolated, and if I rotate the prism, I can throw the various colours in succession on the slit in the screen-aperture, so that the image on a second screen is taken through the succession of spectrum colours from red to violet. Starting with it at the red, and placing a second prism in position behind the screen-aperture, the rays are deviated, as we should expect; but there is no elongation of the image this time, nor is there any diversity of colour. The red image remains red. The same applies to any other colour of the spectrum that is passed through the screen-aperture. But as the successive colours are passed, from red to violet, the deviation on the second screen continually increases, the red being the least deviated and the violet the most.

This experiment shows essentially the same thing as the experiment of crossed spectra, but more perfectly.

The important point is that having once analysed white light into its prismatic constituent colours, this analysis is final, and further prismatic analyses can do no more. Further, each colour has its characteristic refrangibility, which will show itself at every refraction. Newton attached the greatest importance to this experiment, which he called, in Baconian phrase, the "experimentum crucis".

Having effected the analysis of white light into its constituent colours, he proceeded to reverse the process, and to show how the colours of the spectrum could be recompounded into white light. This he did by projecting the spectrum upon a large lens (Fig. 6), when the various rays were all converged to the same place and produced a patch of white light at the focus.

If a kind of large-scale model of a comb as used by Newton (Fig. 7) be placed in front of the lens, some part of the spectrum will be intercepted by the comb, and the white patch produced at the focus of the lens by recombination of the entire spectrum loses its whiteness, and becomes tinged with colour. The colours produced in this way are not pure spectrum colours, because any tooth of the comb permits colour from parts of the spectrum on either side of it to pass. The colours therefore are composite, but the complete spectrum is necessary to produce white. If the comb is moved slowly to and fro, the coloured patch varies, being now reddish, now yellowish, now bluish, but never white. If, however, it is moved to and fro as rapidly as may be, all the spectrum colours are present in succession. They blend by persistence of vision, and the patch becomes white again.

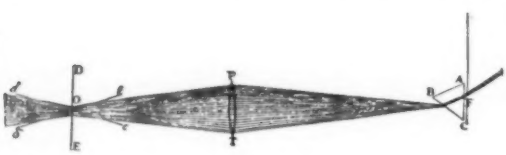

Fig. 6.

Newton devoted considerable attention to the colours of natural bodies, and he came to the conclusion that a coloured body is only coloured in virtue of its power of reflecting some of the components of white light more than others. When a spectrum is projected on a red screen, we see red light where we saw it before, and nowhere else. The colours are not altered, though red is the only one which is strongly reflected by the red screen. The others are mostly absorbed, and their place in the spectrum is comparatively dark. Red is advantageous for such experiments, as red pigments are much the purest in colour.

In another of Newton's experiments the spectrum is projected on a large lens as before. If a white card is held at the focus, it appears white, for all the colours are converged upon it. If a card painted with cinnabar (vermilion) is placed there, it appears red as usual. If we block out the red part of the spectrum at the lens, the card shows no trace of red, becoming comparatively dark. If we block out everything but red, the card shows up with a purer red than at first.

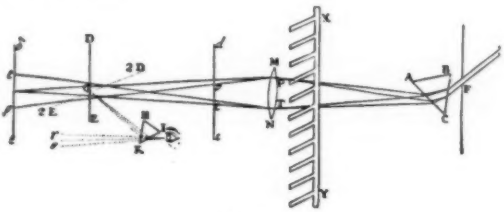

Fig. 7.

We have now been through most of the experiments by which Newton supported his main proposition "The light of the sun consists of rays differently Refrangible", following his own methods as closely as might be. They are a model for all time of how experimental research should be conducted, and it is difficult to our generation to see how any intelligent person could refuse his assent. This was by no means the unanimous opinion of his own contemporaries. One critic, Linus by name, maintained that the spectrum could only be seen when the sun was shining through cloud, and it is strange, but true, that Newton was seriously discomposed by this ridiculous mare's nest. Goethe bitterly attacked Newton, using abusive expressions, and apparently thinking that to use isolated beams of sunlight admitted into a dark room was treating Nature in an unfair manner, and compelling her under torture to say what she did not really mean!

The opposition to Newton's views did not finally die down until within measurable distance of our own time. In a book dated 1853, and dedicated to a man whom I as a boy personally knew, Sir David Brewster says: "The conclusion deduced by Newton is no longer admissible as a general truth 'That to the same degree of refrangibility ever belongs the same colour and to the same colour ever belongs the same degree of refrangibility'."*

* It is not intended to criticize Brewster adversely. The appearances by which he was misled are extremely deceptive.

Let us not make the mistake of thinking that Newton in his optical researches, exploring virgin ground, had easy triumphs. He did not escape the usual fate of discoverers. His jealous contemporary Hooke said his conclusions were not new; his admiring biographer, Brewster, concluded that they were not true!

Exhibits Illustrating some of Newton's Experiments

Arranged by Lord Rayleigh, as a supplement to those shown in his lecture.

"Opticks." Book I. Experiment I.

To show that lights which differ in colour differ also in refrangibility.

Newton's account abbreviated:

"I took a black oblong stiff paper. One part I painted into a red Colour, the other into a blew. This paper I viewed through a Prism of Solid Glass. I found that if the refracting Angle of the Prism be turned upwards so that the paper may seem to be lifted upwards by the Refraction, its blew half will be lifted higher by the Refraction than its red half. But if the refracting Angle of the Prism be turned downwards, so that the Paper may seem to be carried lower by the Refraction, its blew half will be carried something lower than its red half."

A.—"Opticks." Book I. Part II. Experiment XVI.

In this experiment a uniform field of light is viewed through a reflecting prism. Newton used the cloudy sky. An illuminated opal glass is substituted. The bright part of the field is the region of total reflexion. The relatively dark part of the field, corresponding to steeper angles of incidence, is the region of partial transmission and partial reflexion. A blue arc marks the limit of total reflexion. This shows that white light contains a constituent (blue) which begins to be totally reflected at angles of incidence less steep than are necessary for the other components.

"This blue colour being made by nothing else than by reflexion of a specular superficies seems so odd a Phenomenon, and so unaccountable for by the vulgar Hypothesis of Philosophers that I could not but think it deserved to be taken notice of."

B.—A supplement was arranged to this exhibit, allowing the transmitted light to be examined. A pair of right-angled prisms is used, forming a cube after the manner of Newton, reflexion occurring at the diagonal interface. The field is divided into a bright and a dark part as before, the dark part (no transmission) being the area of total reflexion. A reddish-yellow arc marks the limit of transmission, which is complementary to the blue arc seen by reflexion.

This experiment was performed by Newton in a more elaborate way, using the sun's light and projecting a spectrum of the beam transmitted by the cube.

"Opticks". Book II. Part I. Observation xiii.

An experiment showing that Newton's rings, formed between a spherical surface of large radius and a flat plate, are larger in red light than in blue. The observer can rotate the spectrum-forming prism and see the rings dilating as the colour is changed from blue or blue-green towards red.

"Appointing an assistant to move the Prism to and fro about its Axis, that all the Colours might successively fall on that part of the Paper which I saw by reflexion from that part of the Glasses where the Circles appeared, so that all the Colours might be successively reflected from the Circles to my Eye whilst I held it immoveable, I found the Circles which the red light made to be manifestly bigger than those which were made by the blue and violet. And it was very pleasant to see them gradually swell or contract according as the Colour of the Light was changed."

Newton recognized that this proved that there was a coarser structure associated with red light than with blue. In Query 13 at the end he says:

"Do not several sorts of rays make vibrations of several bignesses, which according to their bignesses excite sensations of several colours?"

An Experiment on Frictional Electrification. *From the Minutes of the Royal Society*. 9 *December* 1675.

"That [Newton] having laid upon a table a round piece of glass about two inches broad, in a brass ring, so that the glass might be about one-third of an inch from the table, and the air between them inclosed upon all sides after the manner as if he had whelved a little sieve upon the table: and then rubbing the glass briskly, till some little fragments of paper, laid on the table under the glass, began to be attracted and move nimbly to and fro; after he had done rubbing the glass, the papers would continue a pretty while in various motions; sometimes leaping up to the glass and resting there awhile; then leaping down and resting there, and then leaping up and down again; and this sometimes in lines perpendicular to the table sometimes in oblique ones; sometimes also leaping up in one arch and down in another divers times together, without sensible resting between; sometimes skip in a bow from one part of the glass to another, without touching the table; and sometimes hang by a corner and turn often about very nimbly, as if they had been carried about in the midst of a whirlwind; and he otherwise variously moved every paper with a diverse motion. And upon sliding his finger on the upper side of the glass, though neither the glass nor inclosed air below were moved thereby, yet would the papers, as they hung under the glass receive some new motion inclining this or that way, according as he moved his finger.

The experiment he proposes to be varied with a larger glass placed farther from the table, and to make use of bits of leaf gold instead of papers, esteeming that this will succeed much better, so as perhaps to make the gold rise and fall in spiral lines or whirl for a time in the air, without touching the table or glass.

Ordered that this experiment be tried the next meeting."

A.—An apparatus with flint glass as used by Newton. Its action is uncertain, depending on the hygroscopic condition of the glass. It may fail, as it did when first tried at the Royal Society. Application had to be made to Newton for further directions. In dry weather and with vigorous rubbing it works admirably.

B.—An apparatus with silica glass. This can be taken out and warmed over a spirit lamp; it will then work without fail.

C.—An apparatus with a sheet of celluloid replacing the glass. This will work at any time without fail, if lightly stroked with the dry hand.

NEWTON AND THE SCIENCE OF TO-DAY

By SIR JAMES JEANS, O.M., F.R.S.

THREE hundred years have elapsed since Newton was born, and 276 since his fertile mind had conceived most of those fundamental ideas, the development of which was to add such lustre to his name and be of such outstanding value to science: in astronomy the idea of universal gravitation; in physics the theory of colours; in mathematics the differential and integral calculus (or, as he would have said, the direct and inverse fluxions) as well as the binomial theorem and the method of infinite series. He tells us he had thought of all these before he was twenty-four years old, although he wrote little about most of them until many years later.

The intervening years have seen Newton's ideas examined, tested and developed as no other set of scientific ideas ever has been, and it might seem that by now we ought to be able to assess his greatness and place him in his rightful position relative not only to the science of to-day, but also to that of all time.

We have no doubts as to his greatness, but we probably feel less confidence in our powers to assess his ultimate position in science than we should have done fifty years ago, and certainly less than his immediate successors felt. For they were not content merely to claim outstanding greatness for Newton; they went on to claim a quality of finality and uniqueness which we know better than claim for him to-day.

Laplace, for example, wrote that the "Principia" was assured *for all time* of a pre-eminence above all other productions of the human intellect, while Lagrange, after expressing himself in similar terms, went on to say that Newton was "not only the greatest genius that had ever existed, but was also the most fortunate, for, as there is only one universe, it can fall to only one man in the world's history to interpret its laws".

Such eulogies as these are in striking contrast with Newton's own modest estimate of himself. As his life was drawing to its close, he made the much quoted remark that to himself he seemed only like a boy playing on the sea-shore, "and diverting myself," he said, "in now and then finding a smoother pebble or a prettier shell than ordinary, while the great ocean of truth lay all undiscovered before me".

Which of these two estimates, we may ask, is the nearer to the truth? And how far is it possible to reconcile them? Newton's contemporaries obviously could not have reconciled them. Can we?

All work in mathematics, no matter how modest it may be, may claim immortality if only it is accurate, since absolute truth can have nothing to fear from the disintegrating influence of time. But it is different with work in astronomy or physics. This must at best be relative to the knowledge, or even to the hypotheses, of its time, so that an increase of knowledge or an abandonment of hypotheses may at any time show it to be of no permanent value. This is as true of the work of Newton as of that of lesser men.

Yet Newton's contemporaries, as well as many generations after them, made the mistake of supposing that Newton's work in astronomy and physics possessed the same sort of absolute validity as his discoveries in mathematics; they thought that no future scientist would ever be able to penetrate to the innermost recesses of Nature's temple and discover more of her fundamental mysteries, because Newton had been there already and had seen all that there was to see. But we of to-day know that Nature's temple has many chambers, room after room, and that Newton had scarcely penetrated farther than the ante-chamber. Our thoughts turn to the work of Planck, Rutherford and Einstein. The ante-chamber which Newton had explored was familiar ground to them, but they found the keys to other, and hitherto unsuspected, rooms; in these they discovered laws every bit as fundamental as any that Newton discovered. We see that Newton was wiser than his contemporaries in his vision of the great ocean of truth which lay all undiscovered before him. But does this add to his intellectual stature or diminish it? Should it make him seem greater or less in our eyes?

There are some—although mostly laymen in science—who see science primarily as something that is for ever changing. For them the science of any period is like the sand-castles that the children build on the sea-shore; the rising tide will soon wash them away, and leave the sands clear for the new array of castles which will be built the next day. Those who hold such views are led, somewhat naturally, to make such statements as that Newton is out of date and superseded.

But the comparison is obviously faulty. Science is knowledge, and the primary characteristic of knowledge is not that it is for ever changing, but that it is for ever growing. Of course, all growth implies change, so that science must continually change—like the tree which is for ever budding out in new directions—but this is only a secondary effect. The correct comparison is not with sand-castles, which change because they are continually washed away and replaced, but with a vast building which changes as one floor is built on top of another, or a new wing is built where none stood before. This building is not like a medieval cathedral, the creation of a great number of artificers, each building after his own taste and fancy. It is an embodiment of scientific truth, and the truths of science are the same, no matter who discovers them. Whatever artificers build the structure, the blue-prints have previously been drawn by Nature herself, so that, had Newton lived or not, the building must at some time or other have looked pretty much as it looks to-day. And if we are to estimate how much modern science owes to Newton, we must not only consider how much Newton built himself, but also by how much he expedited the building—to what extent, to put it crudely, did Newton act as a catalyst to the growth and consolidation of scientific knowledge?

Let us first consider the significance of Newton's work in pure mathematics. There have been instances in this subject—especially in the theory of numbers—where a worker has stated a theorem which he believed to be true, but which centuries of discussion have not succeeded in either proving or disproving. Newton's mathematical work was not of that type. His *métier* was not the discovery of fanciful but comparatively useless theorems in recondite branches of

pure mathematics, but the forging of practical tools which were urgently needed for further scientific progress, and we may be sure that if Newton had not forged them someone else would have done so in time.

For example, Newton hit upon the binomial theorem in two distinct ways. He, of course, knew the expansions of $(1 - x)$ raised to integral powers—squares, cubes, etc.; these are easily found by simple multiplication. Starting from these known expansions, Newton interpolated to find the values of the coefficients when $1 - x$ was raised to intermediate values, such as $\frac{1}{2}$, $\frac{3}{2}$, $\frac{5}{2}$. . . . He also calculated the value of $(1 - x)^{1/3}$ by taking the square root of $(1 - x)$ by the simple rules of schoolboy arithmetic, and of course found that the result confirmed what he had already obtained. It is hard to imagine that such simple artifices could have remained untried for long; the binomial theorem was simply a nugget lying on the direct road of progress, and it is quite inconceivable that generations of mathematicians could have passed it by without noticing it.

It was much the same with the greater discovery of the method of fluxions. Rouse Ball tells us that, before Newton appeared on the scene at all, the method had been foreshadowed in the writings of no fewer than seven mathematicians—Napier, Kepler, Cavalieri, Pascal, Fermat, Wallis and Barrow. In the more convenient and practical form of the differential calculus, it was actually discovered by Leibniz in 1675, and published to the world in 1684—nine years before Newton published his fluxional calculus. Apart from the personal questions involved, the bitter controversy as to whether the two discoveries were entirely independent, or whether Leibniz got ideas from seeing Newton's earlier letters on the subject, seems to me relatively unimportant; the calculus was simply another nugget lying on the direct line of scientific advance, and someone was bound to notice it and pick it up—if not to-day, then to-morrow; if not Newton, then Leibniz.

When we turn to consider Newton's still more famous work in mechanics and astronomy, we find the same story repeated; he did not drag science after him into peculiar paths of his own choosing, but kept to the broad main road, discussing the same problems as others were already discussing. The fundamental ideas with which he worked were not in any way novel, or peculiar to himself. His first law of motion had been given by Descartes in 1644, and indeed Plutarch had given it fifteen hundred years earlier together with a foreshadowing of the general principles of planetary motions, and the conception of gravity extending as far as the orbit of the moon. In his "De facie in orbe lunae", Plutarch had written, "You fear that the moon may fall, on the grounds that only light air circulates under the moon and that this is not adequate to bear a solid weight. But the moon is secured against falling by her motion and the swing of her revolution—just as objects put in slings are prevented from falling by the circular whirl. For everything is carried along by the motion natural to it, if it is not deflected by anything else."

Much the same, again, may be said about Newton's famous law of gravitation. The attraction of lodestone for iron had familiarized men with the idea that matter could attract other matter across empty space and, so far back as 1600, Gilbert in his "de Magnete" had conjectured that the earth might attract the moon like a magnet. Kepler had gone further in the same direction in 1619; we find him insisting on the mutual attraction of all matter, instancing the tendency of all objects to fall towards the centre of the earth, as well as the ocean tides which, he maintained, were caused by the attraction of the moon. He said that two stones out in free space would approach one another, like mutually attracting magnets, and would finally meet at the point which we now call the centre of gravity of the two. He thought that the planets were kept moving in their orbits by some power inherent in the sun, and, although forming a very wrong idea of the nature of this power, he conjectured that it must vary as the inverse square of the distance. However, he immediately abandoned this law in favour of one varying inversely as the simple distance, a change which was challenged by Bouillard, who insisted that the true law must be that of the inverse square. Indeed, one of Kepler's own laws—that connecting the periodic times of the planets with their distance from the sun—shows clearly that the true law cannot be other than the inverse square law. Newton tells us that he had already noticed this in the plague years 1665 and 1666, but he did not publish it until it had been noticed independently by Hooke, Halley, Huygens and Wren.

Looking back on all this, we can scarcely accept Wordsworth's description of Newton's mind as

> "for ever voyaging through strange seas of thought alone."

Except in optics, where he voyaged alone because everyone else had lost the way, Newton's ideas were very much those of his contemporaries. His main achievement was not, as is so often stated, that he was the first to think of gravity as extending to the orbit of the moon, for this had been thought of fifteen hundred years before he was born. Neither was it that he was the first to think of universal gravitation, for many had thought of it before him. Neither was it that he was the first to conjecture that the force of gravitation must fall off as the inverse square of the distance, for this idea was generally current in his time. It was not even that he was the first to understand the general principle of planetary motions, for others understood these equally well.

Newton's great service to science, and through science to the human race, was not that he for ever voyaged through strange seas of thought alone, but that he voyaged through familiar seas of thought—if not in company with others, at least in seas that were much frequented by others. Bacon had written of his own contributions to scientific thought, that they did not originate in any mental powers of his own, but rather in the spirit of his age—"partus temporis potius quam ingenii". It was this same spirit of the age that moulded the form of most of Newton's contributions to science. Scattered scientific ideas—and unproved conjectures in particular—were floating around in abundance, and lesser lights of the firmament had made small, hesitating and uncertain advances in many directions. The age was crying for a man who could systematize, synthesize and extend the whole, and it found him in superlative excellence in Newton. But beyond all this, mental powers such as Bacon had disclaimed for himself were present in overflowing measure in Newton—"qui genus humanum ingenio superavit".

His special talents, as I see them, were: first that, out of a large mass of confused ideas provided largely by others, he was able, with his clear and acute mind,

unerringly to pick out the true and discard the false; second that, having done this, his amazing mathematical ability enabled him to replace conjecture by proof, and so provide a firm basis from which he could unhesitatingly advance; third that he did advance, with quite incredible speed, sureness and directness, his supreme mathematical ability enabling him to outstrip all competitors with the utmost ease. From *conjecturing* that a thing is so to *knowing* that it is so, is usually a very long journey; it is one which Newton was so often able to take when his competitors were not. For example, Kepler had *conjectured* that the law of gravitational force might be the law of the inverse square; Newton *knew* that it could not be anything else. Kepler was so far from certain knowledge that he immediately abandoned this conjecture for another; Newton could not be misled into so doing, because his mathematical investigations provided him with convincing proof of the truth of his ideas. Having once settled his fundamental ideas and feeling full confidence in them, he was able to stride forward into the unknown with sure and rapid steps until he had unravelled almost the whole mystery of the then known universe.

In general it was not in originating ideas but in developing them that Newton's greatness showed itself most outstandingly; he was primarily a synthesist and systematizer. Yet here, as ever when we try to compress his genius into a formula or to map out its boundaries, an exception turns up to suggest that it overflowed all limitations; here it is the optics.

If we wish to understand the fundamental cause of his success, we cannot do better than read what Macaulay wrote in his "History": "In Isaac Newton two kinds of intellectual power which have little in common, and which are not often found together in a high degree of vigour, but which are nevertheless equally necessary in the most sublime departments of physics, were united as they have never been united before or since. In no other mind have the demonstrative faculty and the inductive faculty coexisted in such supreme excellence and perfect harmony." Einstein delivers a similar judgment even more forcibly: "In one person he combined the experimenter, the theorist, the mechanic and, not least, the artist in exposition. He stands before us, strong, certain and alone: his joy in creation and his minute precision are evident in every word and in every figure." And in truth he was unmatched both in the breadth and depth of his powers. Both contributed to his success. But if anything, the breadth was more in requisition, and so made the greater contribution to the success.

Yet if we are to estimate the extent to which Newton, with his outstanding mental power and unique combination of talents, expedited the growth of scientific knowledge, we cannot but place to the other side of the account his unfortunate habit of keeping his results to himself until they had been discovered independently by someone else. To some extent, this also may be ascribed to the spirit of the age. The seventeenth century had but little of our vision of science as a great benefactor to the human race, or of new knowledge as something which ought to be disclosed at once so as to expedite further progress by others. A man's scientific discoveries were his private property, with which he could do as he liked. Often his ambition was not to enable others to benefit from them, but to prevent others benefiting from them—this is why he would often publish his results in cypher. Or he might regard science, and mathematics in particular, as a sort of mental gymnasium in which contests of skill took place, and competitors issued challenges to one another to determine which of the two was the stronger or quicker. To use Newton's own metaphor, science was, more than to-day, a matter of playing with pebbles on the seashore, and seeing who could find the smoothest pebble or the prettiest shell. Thus there was less feeling of moral responsibility than now, perhaps even less interest in science for its own sake.

So we see Newton's terrifically powerful mind playing with the problems of science like a cat which plays with a mouse and loses all interest when he has killed it, or as we play with a crossword puzzle and regard the incident as finished when we have solved it.

We can see this exemplified in nearly all Newton's major discoveries. He tells us that he had thought of the method of fluxions in the plague year of 1665. Yet before publishing it even partially (except in cypher) to the world, he allowed twenty-eight years to elapse—years in which Leibniz discovered and published the same thing—and it was not fully published until 1736. Again in this same year of 1665, Newton satisfied himself that a force of gravity obeying an inverse square law explained the motion of the moon "pretty nearly", and was content to leave it at that. Later, he found that this same law would result in the planets describing the exact orbits they were known actually to describe—ellipses with the sun in one focus. Again he was content to leave it there. Most men would have published this as proof positive of the law of the inverse square, but Newton kept it to himself until Halley travelled to Cambridge to consult him on this very problem. Newton then explained that he had solved the problem five years previously, but had mislaid his proof. Finally, Prof. Andrade has reminded us of the reluctance with which Newton published his "Principia"—probably the greatest single work of the human intellect—and of the pressure which had to be applied before he would do so.

Apart from the general secretiveness of his age, we may perhaps find three further causes for this delaying tendency in Newton: an over-active mind which refused to halt by the wayside to perfect and write out results, a disinclination to be satisfied with anything short of perfection, and an extreme sensitiveness to criticism and consequent desire to avoid controversy, this resulting more than once in a disinclination to pursue the study of science any further. In spite of these handicaps Newton bestrode the scientific world like a Colossus: what would he have been without them?

Such was the man as I see him; let us now consider how much of the scientific edifice which Newton built, or was built under his influence, is still in useful service to-day.

Outside pure mathematics, Newton's procedure was simply that of every other true man of science. He started from known facts which, in his judgment, seemed likely to be related—as for example, if legend can be trusted, that apples fall to the ground with an acceleration of 981 C.G.S. units, and that the moon circles round the earth once every 27⅓ days—and tried to represent the isolated facts of such a group as special instances of a general system of laws. If this could be done, the set of laws so discovered could be made to predict other phenomena, which could then be submitted to the test of experiment or

observation. If the laws passed this test, they were assumed to correspond to something in ultimate reality; provisionally at least they were true laws, and showed how Nature worked. If Newton differed from others, it was in the greater range of facts on which he worked.

In any scientific era, the search for such sets of laws is clearly restricted by the range of facts which are known, or are capable of being known, at the time; and we cannot expect scientific men to discover laws of a higher order of accuracy than the observations through which the laws are discovered.

Some may challenge this, arguing that a genius of sufficient stature ought to be able to discover the true reality behind Nature from quite meagre evidence —"ex pede Herculem"—and that, when this reality was known, the laws governing its behaviour could be deduced and would necessarily be true throughout the whole range of phenomena, whether known or still to be discovered. Unhappily, Nature is not like that. Science can, from the nature of things, never attain to a knowledge of the realities of the world, but only to a knowledge of the phenomena—of the impressions that the world makes on our senses, generally, of course, through instruments of precision. Our sense-organs form a sort of screen on which Nature's lantern is for ever projecting pictures; we can study these pictures, but can never pass behind the screen to see how the lantern works. As the sequence of pictures thrown on the screen is all that we can ever know or study, a set of laws that links the pictures together in a perfect systematic order is the most that science can provide, or can properly be asked to provide. If meagreness of the instrumental equipment in any age prevents the science of that age from knowing the finer details of the pictures, then we cannot expect the science of the age to provide laws governing these fine details.

For this reason we have to introduce a sort of relativity into our conception of the aims of physics; we may say that the science of any age can aim no higher than at predicting the result of any experiment that can be performed *in the age in question*. If this is conceded, we may claim that by far the greater part of Newton's work was accurate and final, relative to the age in which he lived; and this part is still of value and still in use to-day.

The enormous increase in instrumental power which has occurred since Newton's time has thrown open new fields for exploration in two directions—towards the infinitely great and towards the infinitely small, from the electron at one end of the scale to the nebulæ and the whole of space at the other. Man and the man-sized world with which Newton was mainly concerned lie about midway between these two extremes.

The laws of Nature are of course universal, so that the same set of laws must prevail throughout the whole of this range, but different aspects of these laws assume importance in turn in different parts of the range. So much is this the case that we may almost regard the different parts of the range as constituting separate and detached worlds in which completely different sets of laws prevail. There is the small-scale world of electrons and of atomic physics in general in which the laws of quantum mechanics prevail, the man-sized world in which the laws of molar mechanics prevail, and the world of the great nebulæ in which the laws of relativity prevail. These three worlds are all governed by the same laws, but factors which are all-important in one become mere insignificant corrections in the others. Newton's work was applicable almost exclusively to the middle-sized world of molar mechanics, and so not in general to the other two worlds, the existence of which was scarcely suspected in his day. Lagrange was wrong in thinking that there was only one universe, and that it could fall to only one man to interpret its laws; there are worlds within worlds, and Newton had only interpreted the laws of one of these. Relative to this world most of Newton's work was accurate and final, which means that it was also accurate and final relative to the age in which he lived.

If we examine any recent paper on theoretical physics, we are likely to find it plentifully sprinkled with either the symbol h or the symbol c, or both. These two symbols were unknown to Newton, at least in the sense in which we now use them; they are, so to say, the emblems of the theories of quanta and relativity which did not invade physics until the present century.

The symbol c denotes the velocity of light, which is greater than the velocity of any material object, and is usually enormously greater. Also in dynamical problems the ratio of these two velocities can only enter through its square. We are now familiar with electrons which move almost as fast as light, but the kind of dynamics with which Newton was concerned contemplated no velocity greater than that of the planet Mercury, which is about 48 km. a second. For this, the ratio of the squares of the two velocities is 0·0000000256, or, to seven significant figures, is *nil*. If we disregard this fraction, the dynamics of relativity becomes absolutely identical with that of Newton, so that to seven significant figures Newton's dynamics was adequate and factually perfect for an age in which nothing moved faster than the planet Mercury.

It is the same with the h of the theory of quanta. This measures the atom of action which Planck discovered in 1899, and although it is of the utmost importance to phenomena on the atomic and subatomic scale, it is absurdly small in comparison with the amounts of action involved in the activities of everyday life. Roughly speaking, the ratio is that of an atom to a gram, or of a gram to a star. This small quantity again was quite negligible in all the problems with which Newton was concerned.

Thus the Newtonian mechanics, dynamics and astronomy were absolutely right factually, except for quantities which were so small that they could not either affect Newton's problems in any way, or make their existence known through the best instrumental accuracy that was available in his day. To see Newton's work in these subjects in their proper perspective, we must think of him as the builder of the ground floor of the great building of mechanics, dynamics and astronomy, and as its architect for many floors above this. Newton's first and last serious contribution was his "Principia", but after this, floor after floor was added on strictly Newtonian lines: on the dynamical side, the generalized dynamics of Lagrange, Hamilton and Jacobi; on the astronomical side, the dynamical astronomy of Laplace and many others; on the physical side, the electrodynamical theory which we associate primarily with Clerk Maxwell. All this and much more followed the Newtonian architectural plan without any deviation whatever for two centuries. It was not until 1887—just two hundred years after the appearance of the "Principia"—that the Michelson-Morley ex-

periment first showed that the Newtonian scheme was in actual fact imperfect. But the experiment had to be discussed for eighteen years before this inference was drawn, and it was only when Einstein's restricted theory of relativity appeared that the Newtonian scheme was generally recognized to be inadequate.

It is, however, inadequate only with reference to the ultra-refinements of modern science. When the astronomer wishes to prepare his "Nautical Almanac", or to discuss the motions of the planets, he uses the Newtonian scheme almost exclusively. The engineer who is building a bridge or a ship or a locomotive does precisely what he would have done had Newton's scheme never been proved inadequate. The same is true of the electrical engineer, whether he is mending a telephone or designing a power-station. The science of everyday life is still wholly Newtonian; and it is impossible to estimate how much this science owes to Newton's clear and penetrating mind having set it on the right road, and this so firmly and convincingly that none who understood his methods could doubt their rightness. Without his guidance, smaller men might have argued for centuries as to what was right and what was wrong. When Newton had spoken, it soon came to be recognized that the time for controversy as to which was the right road was past; it only remained to advance along the Newtonian road.

Newton's work in optics stands somewhat apart from the rest, in that he was much less indebted to his predecessors and contemporaries. Indeed, current ideas on the nature of light were so vague and erroneous that he was probably hindered rather than helped by what he knew of them, and was perhaps fortunate in not knowing more. We know that he attended the lectures of Barrow in Cambridge, where he would doubtless become acquainted with the optical theories of Descartes as well as with those of Barrow himself. Now Descartes thought that all space was filled with substance, and that vision resulted from the transmission of a pressure from one particle to another of this substance—much as a blind man pokes about with a stick. He thought that different sensations of colour resulted from the particles rotating with different speeds, red resulting from the most rapid rotations, and yellow, green and blue following in this order. Barrow held even more fantastic opinions; he thought that red light was more concentrated than light of other colours, but was broken up by intervals of complete darkness, yellow light consisted of a mixture of this red light with white, and so on. Hooke conjectured that light consists of a rapid vibrational motion of an ethereal medium filling all space, different colours being produced by different shapes of wave; blue, for example, resulted from "an oblique and confused pulse of light".

Into the midst of all this confusion of thought came Newton, to whom, in Einstein's words, "Nature was an open book, whose letters he could read without effort. The conceptions which he used to reduce the material of experience to order seemed to flow spontaneously from experience itself, from the beautiful experiments which he arranged in order like playthings." With the prism he had bought at Stourbridge Fair, he went at once to the crucial experiment of the spectrum. Finding that this was longer than it was broad, he at once saw that differences of colour must result from different degrees of refrangibility. This discovery alone, the subject of Newton's first scientific paper, published in our *Phil. Trans.* of 1672, at once took the theory of light leagues beyond the point to which any previous investigator had brought it, and opened the road to further rapid progress.

But Newton brought his views into discredit in the eyes of many by appearing to think of light as a material substance—Hooke definitely brought this charge against him. Actually Newton repeatedly claimed that his results did not depend on any special views as to the ultimate nature of light, and he was careful to avoid such words as 'corpuscles' which might seem to imply such views. Yet much of his thought seems to become meaningless unless we identify his rays—the "least parts" of light—with something of a corpuscular nature. However little he may have said about it, Newton seems always to have had his own picture of the structure of light before his eyes, and this picture was corpuscular.

For two centuries many thought of this as the one serious mistake that Newton ever made. Recently the pendulum has swung—perhaps too far—in the other direction. Light is a transfer of energy from matter at one place to matter at another, and this transfer can only take place by complete units, or 'quanta'. This led Einstein to picture light as a shower of arrows, each arrow containing a single complete quantum of energy, and some have hailed Newton's corpuscular theory of light as a brilliant prevision of this.

But we must remember that Newton introduced his corpuscles primarily to explain the rectilinear propagation of light, whereas Einstein introduced his light-arrows to picture the quite different property of atomicity. This being so, the similarity of the two pictures can scarcely be quoted as a supreme example of intuition on the part of Newton. It might be if light really did consist of corpuscles; Newton would then have arrived at the truth by one road two hundred years before Einstein reached it by another. But no competent physicist can for a moment suppose that light consists either of corpuscles or of light-arrows; these are only pictures we draw for ourselves to help us understand parts of a truth which we know, as a whole, to be for ever beyond our comprehension.

At this point we are inevitably brought to consider Newton as a philosopher. Unlike his great predecessor Descartes and his great contemporary Leibniz, he seems to have had very little interest in philosophy for its own sake. He was what we might describe to-day as a pragmatist; his main concern with a philosophy was not as to whether it was true, but whether it worked and yielded useful results. It was another illustration of the ever-recurring difference between the British and the Continentals.

Descartes had taught mathematicians to measure co-ordinates from fixed lines on a piece of paper. Newton wanted to be able to measure mechanical and astronomical co-ordinates in the same way from positions in space and instants in time. So he assumed without further ado that time could be measured from a fixed standard instant, and that the remotest parts of the universe contained vast immovable masses, providing standard positions against which absolute motion could be measured.

This assumption of the existence of absolute time and space was a mere working hypothesis which, as he admitted, might not be true at all: "it may be that there is no body really at rest, to which the places and motions of other bodies can be referred". But the assumption, whether true or false, produced

results—nothing less than a consistent scheme which explained a large part of the then known workings of Nature. This being so, philosophers such as Bruno and Leibniz might argue in vain that absolute space, time and motion were meaningless and illogical.

It was much the same in mechanics. In the man-sized world of seventeenth-century science, motion was the result of pushes or pulls of the kind that a horse exerts on a cart. Other suggested causes of motion, such as action at a distance, were regarded much as we of to-day regard levitation or table-turning, and described as "occult qualities". It was inevitable that Newton's force of gravity should come under this description. Leibniz wrote, "Some men begin to revive, under the specious name of forces, the occult qualities of scholasticism, but they bring us back again into the Kingdom of Darkness". Newton's reply was a fine defence of his pragmatical philosophy. To understand the motions of the planets under gravity, he says, even though we do not know the cause of gravity, is as good progress in philosophy as is a knowledge of the motions of the wheels of a clock in the philosophy of clockwork, even though we do not understand why the weight which moves the wheels falls earthward.

At the same time, he refused to admit that his gravitational forces were not wholly mechanical. From the celestial phenomena, he said, we calculate the force of gravity with which bodies are drawn to the sun and the several planets. Once having found these, we deduce the motions of the planets, the comets, the moon and the sun, and "I wish we could derive the rest of the phenomena of Nature by the same kind of reasoning from mechanical principles".

It is interesting to speculate as to what might have happened if Newton had shared the views of Leibniz as to the relativity of all motion. It is conceivable that he might have arrived speedily at what we now call the restricted theory of relativity, that through it he would have seen the impossibility of action at a distance, and would then have developed a relativity theory of gravitation similar to that of Einstein. It is conceivable, but I can scarcely think it would have happened—the necessary physical ideas and mathematical technique seem to be centuries removed from the fields in which Newton worked. It is more likely that he would have been compelled to come down to an approximation which would have brought him back to the Newtonian mechanics and the inverse square law. If so, his erroneous philosophical background of absolute space and time was a real advantage, since it provided him with a short cut to a region which would have had to be explored anyhow as a preliminary to further progress. It enabled him to go direct to the main point as it appeared in the seventeenth century, neither hindered by any philosophical subtleties nor haunted by any fears that his system might not be an expression of final absolute truth.

In this way Newton created a system which, although philosophically unsound, was destined to endure for two centuries before any chinks were found in its armour. Then more philosophically minded Continentals had to be called in to patch up the deficiencies.

In this quality of practical and opportunist directness, as in many others, Newton seems to me to be typically British. In general he excelled where the British excel, and was weakest where the British are weak. If we think of his various pre-eminences in turn, and consider who runs him closest in each, we shall find it is usually a British man of science who comes to mind. If we try to match the simple directness and the clear logical sequence of his experiments in optics, our thoughts turn to our own Faraday and Rutherford. Clerk Maxwell runs second to him, I think, in other aspects of his work. But if we think of those who excelled him in his philosophical outlook, we have to think exclusively of Continentals, from Leibniz on. His successes were typically British, and his failures—in so far as he ever failed—were also typically British. We can not only be proud that this greatest of all scientific men was our own countryman, but we may reflect with satisfaction that he was no freak genius or sport, but was the concentrated embodiment of all the distinguishing characteristics of British science.

FUTURE OF UNIVERSITY EDUCATION

THOUGH the report (outlined on page 718 of this issue) of the Committee appointed by the British Association to consider university education after the War is only of an interim nature, it contains so much of outstanding and immediate value (if any of its proposals are eventually to be put into effect) that it warrants the close attention of all those interested in formal education and those who appreciate the valuable contributions which education will be able to make towards the new world order.

The Committee is obviously determined to pursue its policy of eliminating the defective and strengthening the weak spots in university education, of modifying certain systems and traditions, and of seeing that those universities which have suffered through the evil effects of actual warfare, or the even worse results of occupation by an unscrupulous enemy, are rehabilitated after the War. It is plainly anxious that all this shall be carried out in such a manner as to bring the maximum benefit not only to the universities immediately concerned but also to the whole international community. We tend at times to look upon university training as a means to an end which affects university men and women only; this is not only a very narrow but also a dangerous point of view, for universities are national institutions just like parliaments, and exist not only for the benefit of those who gain admission to them but above all for the community in general. The best material must be voted into them, and they must continue to make the best of that material so that their final products shall be not only a credit to themselves but also a distinct asset to the whole community.

Having read this interim report, we are satisfied that the Committee is treading the right path. It also indicates certain angles of approach which it proposes to make later on, and these indications, stated or implied, suggest that there will be much more for us to consider seriously when the final report becomes available. For that reason, only certain sections of the present report are sufficiently advanced to warrant useful comment.

The proposal that the 'modal' age of entry into the university should be nineteen is (apart from war-time contingencies) timely and deserving of support. Some of those actively concerned with university education have often expressed concern over the immature type of undergraduate gaining access to the universities. A first-year university student is in no way comparable to a sixth-form secondary school pupil, and the growing tendency to-day for a student to do his nominal first university year while still at school, and thus enter upon his second year when beginning his university career, makes the problem even more acute. When a student enters a university he finds himself thrown very much on his own resources, and often for the first time in his academic life has to make important decisions for himself in spite of university codes and regulations and tutorial supervision. Those university teachers who have had such students under their charge must often have noticed that many of them are too immature and indeed unready for such responsibilities.

Closely knit with the proposal to raise the age of entry, at any rate in its effect, is the Committee's proposal that a student should have given a year of national or international service or have had a year's education abroad before entering upon his university career, for thus will his mind be broadened and his sense of responsibility and of values be strengthened. Thus would students attain that greater appreciation of the value of social intercourse, greater maturity of mind and more experience of life, all of which have been so lacking for years past in our university freshmen. When they enter the university, they should be fit to take immediate advantage of the greater freedom from rules and regulations and of the fact that they are now treated as men and women and not as boys and girls. In present conditions too many fall victims to what is to them a new regime.

The comments of the Committee on entrance tests and scholarships will receive widespread support because such views have been expressed so often before, though, alas, little action has been taken. The present form of entrance examination is soul-destroying. The teacher in the school is tied not only by the examination system, but also by the peculiar requirements of that university which his pupils hope to enter. Though we would be the last to press for absolute standardization at the expense of individual development and expression, we strongly support the Committee's suggestion that there should be some form of standardization of examination so far as general education is concerned. Specific and special knowledge must, if necessary, be tested individually; but such a requirement should be imposed only in exceptional cases, otherwise the evil effects of examinations will always be with us. The time is also propitious for those universities which insist on a pass in certain definitely stated subjects in their entrance examinations to pause to consider whether such provisos (for that is what they really are) are necessary and, indeed, whether such rulings are not harmful. We have no quarrel with the demand for a reasonably high standard in English, mathematics, science, and other subjects of cultural value; but it is difficult to see the reason for making such subjects as Latin or Greek compulsory, as certain universities do. It is well known that in many such cases, candidates, who have never studied Latin before, read enough, after passing their School Certificate examination, in order to pass university entrance, and then begin forgetting all about it. Surely, with all due respect to a sincere study of the subject, this short-term cramming is an unnecessary waste of time and is quite meaningless.

The most potent effect of the Committee's proposals concerning this type of examination will be felt in both directions, that is, beforehand in the schools and afterwards in the universities. A better type of

student would be the universities' share, whereas in the schools the pupils would profit by equality of opportunity and a more all-round training, especially from the social point of view.

It is recommended that all students should reside for at least one year in a university college or a hostel. It is also recommended that every student should be placed under tutorial supervision. These recommendations are complementary. But, though the Committee sets our minds at rest so far as expense is concerned, the difficulties will prove greater than one might on casual consideration think. Such difficulties as might arise will, of course, be least in the collegiate universities and in some of those modern universities, such as Reading and Manchester, which have developed splendidly comprehensive hostel or hall systems. The fraternity and dormitory systems extant in certain American universities would also lessen the difficulties. But a year's residence in a hostel would lose much, if not most, of its value if not lived fully and seriously. This would not be easy in many modern British universities situated in large towns which draw most of their students from their immediate surroundings. The hostel system in such cases would have to be subjected to very strict discipline if outside, especially home and town-life, distractions are not to exert a very adverse influence. Tutorial supervision is practised to a greater or less degree in most of our universities. In some, where it is scarcely anything but nominal, it needs tightening up. Much will depend upon the tutorial staff, who ought to be specially trained for the purpose, since they are more like teachers than lecturers. But the system itself would be greatly helped by some form of internal residence.

We can offer little but praise for the Committee's recommendations for re-planning teaching departments and curricula in accordance with modern conceptions of the interrelation of different branches of knowledge. In fact they have been borne out so recently as December 2 by the Archbishop of Canterbury, in a speech delivered in London, in which he said we should take note of the balance between human and scientific subjects. Dr. Temple considers that the pendulum has swung too far in the direction of scientific training. We tend to produce people whose conscious thinking is almost entirely governed by the methods of the laboratory, and whose minds only move freely when they are dealing with what can be measured, weighed and stated with exact precision. As Dr. Temple claims, this is not true of any human problem.

This carries our minds back to an address made by the same speaker about twelve years ago in Manchester, when he pleaded for more research in the universities. This side of academic life has certainly developed, and there may lie the cause of the problem. Too much of the training of university students is left to research specialists. Research specialists are bound to give prominence to their own pet interests, and thus are often unable to strike a balance for the average student. It is clear that both the narrow specialist and the broad general teacher are required in the university of the future, for very often the best teacher in the university is only a second-rate research worker, and vice versa.

It is therefore with a feeling of deep satisfaction that we note the Committee's insistence on training for world citizenship in all faculties, pass and honours, of our universities. No university is justified in granting a first-class degree to a student who has simply passed satisfactorily through its faculty of science. Social studies and the humanities must play a certain part in the training of all students. This applies especially to science students, for it is almost axiomatic that, in present circumstances, a man might gain a first-class degree in science and be no more appreciative of the essentials of world citizenship than any less fortunate individual who has not gone so far as through a secondary school.

One misleading remark in the Committee's report is that its recommendations for the institution of honours and pass schools of "Philosophy, Natural and Social" would meet the "charges of neglecting the non-vocational education of citizens". It may be true that the ideal university education should not be influenced by vocational demands, but the fact still remains that nearly every university student treats his university career as a basis or qualification for a profession usually chosen beforehand. It is also true that those university departments whose subjects offer little or no vocational value often find themselves practically void of students. In this claim, the Committee seems to be too modest, for surely such a school of "Philosophy, Natural and Social" should prove to be the very best one for the future school teacher. In their examination syllabuses, the universities themselves are in large measure responsible for the narrow and far too specialized training given in our sixth forms; but another reason may be found in the type of honours man usually chosen for teaching such boys and girls. Give him, not an honours degree in one subject, but what the newly proposed course can offer, and then our university candidates from the schools would receive a wider training in sociology and citizenship than they do at present.

The Committee's attempt to link up natural and social philosophy in the history of discovery is deserving of very careful consideration. In our studies of discovery we are recommended to include "natural science or scholarship, studied in relation to social and political history". This should help university, and even school, teachers to break away from the belief that they are doing enough when they bring into their science courses the study of the works of such paragons as Newton, Faraday, Boyle, Darwin and Harvey. In this connexion, teachers are recommended to read Dr. H. A. Mess's "Social Structure", where a considerable amount of background is offered for such courses as are proposed by the Committee. For example, the period between 800 and 400 B.C. was notable for its advances in ethical thought. There we find such names as Lao-Tze, Gautama Buddha, and the Jewish prophets from Amos to the second Isaiah. In Greece during the fifth and fourth centuries before Christ, considerable additions were made to knowledge, especially in

mathematics, physics, medicine and biology. Socrates developed the method of argument, and Aristotle was the founder of formal logic. Rome followed during 100 B.C.–A.D. 400 with contributions to engineering, government and the formulation of law. And so one could go on. This is the history of discovery—not the discovery of natural laws alone, but also of human relations.

It is certain that our science students need a much broader and deeper background to their training. They need to be given training not only in scientific method but also in the interrelation of science with the other cultural disciplines. By giving them this we make them think of themselves not only as wheels in a machine concerned only with their own movements, but also as necessary parts of that machine, the working of which they seek to understand—the machine of human culture.

All this, the British Association Committee clearly bears in mind in its deliberations, and for this it is to be congratulated on a very valuable piece of work, which, nevertheless, is only partially completed. The Committee has, much to its credit, kept in mind the most important needs in our universities of the future, namely, equality of opportunity, freedom of development, thought and speech, the promotion of international collaboration, needs and service of the community—or in one phrase, training for world citizenship.

BRITISH ASSOCIATION REPORT

ONE of the important questions raised at the Conference on Science and World Order, which was held in London during September 1941, under the auspices of the British Association Division for the Social and International Relations of Science[1], was that of post-war university education and the rehabilitation of universities destroyed or damaged during the War. A Committee was consequently formed to examine and report on the problems involved. This Committee, under the chairmanship of Dr. J. C. Maxwell Garnett, is composed of twenty-four members (several representing universities outside the British Empire), together with the president and general officers of the British Association, and Prof. F. E. Weiss and Mr. A. Gray Jones as joint honorary secretaries. The Committee presented an interim report to the Council of the British Association in September 1942, and this has now been published[2].

Entrance to Universities

The Committee recognizes that those who enter upon a university career should do so with a greater maturity of mind and a wider experience of life than has hitherto been the case. The 'modal' age of entry into the universities should therefore be not less than nineteen. During their nineteenth year, students should carry out some approved form of national or international service (civil or military). Where possible, this work should be related to the intended profession of the students of applied science. For example, engineers should do a year of workshop training, and medical students might either enter a hospital or do some other form of welfare work. In other cases, a year abroad could be accepted as one form of international service.

Entrance examinations should consist of two parts: (1) a test of general education; (2) a test of special knowledge. So far as (1) is concerned, an ever-widening group of universities (beginning with all those in the United Kingdom) should adopt each other's tests. This would not apply to (2), but each department of each university should satisfy itself concerning a candidate's special knowledge. Though this recommendation would not be seriously impaired if certain universities still insisted upon every candidate for matriculation possessing, as part of a general education, some one particular kind of knowledge (for example, Greek or Latin) not required by other universities in the group, the Committee expresses the view that the adoption of its recommendations should be subject to no such exception.

Personal character (purposefulness, enterprise, initiative and originality) should receive careful consideration, together with the generally examined knowledge and intellectual qualities of a candidate when he is being considered for a university entrance scholarship. One of the greatest advantages of this, so far as each individual is concerned, would be to increase equality of opportunity. The schools themselves would also benefit in that they would thus be encouraged to provide more all-round training, especially social, by means of out-of-school activities.

For every internal student, at least one year's residence in college or hostel should be enforced. The Committee states that this would not involve a prohibitive cost; but further inquiries are being made. Personal direction and watching of every undergraduate's studies should be more fully developed by the adoption or extension of the tutorial system.

Freedom for discussion should be the keynote of all university education. Freedom of thought and expression, especially so far as world affairs are concerned, should be assured.

An International Auxiliary Language

International communication will be indispensable after the War, and the universities can contribute towards this by requiring all their students to be able to make themselves understood, by speech and writing, in some one auxiliary means of international communication. This is not a question of learning another language but of devoting one long vacation to the acquisition of an auxiliary means of communication. Basic English, for example, has only 850 words, whereas the English language contains 240,000, while French has 100,000 and Italian 80,000 words. A sufficient acquaintance with Basic English could be obtained within a few days by the many post-war students who will be able to talk some English. Professional diplomats can, of course, transact their business in a common language. Since it ceased to be Latin it has generally been French and is likely, in future, to be English. English is one of the two languages of the Anglo-Soviet Treaty and the common language of Generalissimo Chiang Kai-shek and the peoples of India.

The desirability of promoting the introduction of an auxiliary language as an international means of communication was given particular attention after the War of 1914–18, and the position and prospects of this subject were surveyed in a report of a British Association Committee published in 1921. In order to present the problem in a comprehensive manner, the Committee communicated with leading authorities and associations having special knowledge of classical

and modern languages as well as interest in the use of an international auxiliary language. The particular claims of three types of such a language were stated, namely: (1) A dead language, for example, Latin; (2) a national language, for example, English; (3) an invented or artificial language, for example, Esperanto and Ido.

The advantages and disadvantages of each of these types were considered by the Committee, which reached the following conclusions: (1) Latin is too difficult to serve as an international auxiliary language; (2) the adoption of any modern national language would confer undue advantages and excite jealousy; (3) therefore an invented language is best. Esperanto and Ido are suitable, but the 1921 Committee was not prepared to decide between them.

However, since the publication of the British Association's report twenty-one years ago, the great political changes which have taken place, or which will have taken place at the end of the War, point to the fact that any auxiliary means of communication will have to be closely related to the English language. The Universities Bureau of the British Empire, and the new Association of University Professors and Lecturers of Allied Countries in Great Britain[3], might well assist in promoting the movement for an auxiliary means of international communication by preparing a report on the question whether Basic English is suitable for the purpose, or, if not, what alternative is to be preferred.

International Education Organization

Experience in university and other kinds of education gained by the different countries of the world should be pooled by the formation of an international education organization representative of teachers (including university teachers), governing bodies, local education authorities and governments. The wider utilization of graduate scholarships, sufficient to cover passage money and all necessary expenses, should aid the exchange of students between the universities of different countries.

The universities should also associate themselves more closely with non-vocational adult education.

Replanning University Curricula

Another term of reference of the British Association Committee is "To consider the replanning of teaching departments and curricula in accordance with modern conceptions of the interrelations of different branches of knowledge, particularly those of science and the humanities".

University studies are still showing a tendency to split up into a growing number of separate specialisms, very remarkable for their diversity. These tend to become increasingly removed from the life of the community. The Committee has therefore considered two proposals: *A*, a general degree course including both natural sciences and the humanities; *B*, a proposal to include courses of the elements of sociology and citizenship in the already existing specialized schools of natural and applied sciences.

The recommendations concerning *A* are partly derived from the proposals for an Honour School of Philosophy and Natural Science which were considered in 1923 by the University of Oxford and which have recently been informally revived. The crux of the recommendation is the institution of honours and pass schools of "Philosophy, Natural and Social". Here the modern world would be studied as an integrated whole against a background of modern science. The course of study would include the natural and the social sciences with the ultimate aim of constructing (as expressed in Whitehead's "Process and Reality") "a system of ideas which bring the æsthetic, moral and religious interests into relation with those concepts of the world which have their origin in natural science".

In such a course, chemistry and physics would come together under 'physical science', and zoology, botany and physiology under 'biological sciences'. During the study of these subjects, detailed knowledge would be looked upon as being less important than an apprehension of scientific method. Observational and experimental work would, however, have to be included. Along with these courses, there should be courses in the history of discovery, not only of inventions but also of ideas and conceptions. The study of man and society, including the contemporary international community, should be begun in the first year and extend through the next. The last year would then link together all previous work, whether in natural sciences or the humanities, and bring it in relation with the best philosophic thought of the Western world, begun by Plato and Aristotle and continued by the greatest modern thinkers from Descartes to Whitehead.

The Committee gives a warning against having separate tutors directing study of the separate subjects; the whole course must, to be effective, be treated as an integrated whole—an organized unity.

Proposal *A* is designed by the Committee to provide for the "non-vocational education of citizens". Proposal *B* should ensure that the importance of the interrelation of science and the humanities be also recognized in the specialized courses for students preparing for a professional vocation of science. For reasons stated by the Committee (and reiterated time and time again in NATURE and elsewhere), all students in the faculties of science should attend some discussion classes in subjects outside their specialism. Here such students should be addressed by persons who understand their background interest; the students are not recommended to attend courses of lectures in the faculties of arts and commerce.

There should be "special interest" courses; for example, medical students should attend courses on the public health services, vital statistics and human needs. These "special interest" courses should be followed by courses made up of discussion groups of a more general nature on citizenship, social and industrial relations, anthropology, psychology, and other branches of the humanities.

Such courses should cover the student's first two years, and should be enough to enable him to start thinking about social affairs, so that later on in his university career he will take complete advantage of the debating and political societies organized by the students themselves in their unions and guilds.

While claiming that all this is going in the right direction, the Committee warns against forcing the pace. The new teachers with the new outlook must eventually be created. This can be done, (*a*) by teaching general science in all schools, and (*b*) by providing courses in borderline subjects in the universities, such subjects to include social medicine and vital statistics in the faculties of science and medicine, social technology, demography and standards of human needs in the faculties of arts and commerce.

Rehabilitation of War-Damaged Universities

The British Association Committee has under survey the position regarding teaching material, apparatus, books and staff in universities which have been destroyed, damaged, disorganized or closed as a result of the War.

The Committee recommends that the United Nations should, at the time of armistice, require the enemy Powers to make full restoration of university property stolen or destroyed in the countries they have temporarily occupied.

Many scientific instruments and much apparatus now being put to war purposes would be of great value for teaching and research, and the Committee recommends that the Governments of the United Nations either give or lend them to the universities, especially those being rehabilitated after the War, and that they require similar action of the enemy Powers.

The Committee has obtained much information from the London International Assembly concerning war damage to the universities. Lists of up-to-date text-books as well as standard books and works of reference are being compiled. When these are sufficiently complete, they will be sent to British universities and university colleges and to the American University Union. It is then hoped to collect a large number of spare copies of such works. The Committee also recommends that the British Council consider buying certain books, and it is also hoped that publishers may supply, for the use of rehabilitated universities, books at reduced prices. It is also hoped to collect copies of learned journals or microfilms of them. The Committee may be able to arrange for the preparation of such microfilms.

It is the opinion of some members of the Committee, who are citizens of the Allied States in Europe, that it will be necessary for British and American universtities to train teachers and research workers for some of the universities whose staffs have been killed or dispersed by the enemy.

[1] NATURE, **148**, 331, 388, 393, 424, 426, 454, 456 (1941).
[2] "The Advancement of Science", **2**, No. 7, 255 (1942). British Association, Burlington House, London, W.1. 5*s*.
[3] NATURE, **150**, 692 (1942).

RECENT ADVANCES IN OUR KNOWLEDGE OF THE PHOTOGRAPHIC PROCESS*

By DR. C. E. KENNETH MEES, F.R.S.
Research Laboratory, Eastman Kodak Company

WHILE many materials are sensitive to light and can be used for making photographs, the present art of photography is based upon one specific process : the light-sensitive material is silver bromide in the form of extremely small crystals held in a layer of gelatin.

The direction in which advances have been made in recent years is in the elucidation of the structure of the light-sensitive materials and the factors which produce great sensitivity in the silver bromide crystals, so that very small amounts of light are sufficient to make them developable ; in the study of the action of light itself and of the change which occurs in the exposed crystals ; and in the study of the development reaction by which the exposed crystals are reduced to metallic silver.

The nature of the silver halide crystals and the origin of their sensitivity were elucidated between 1920 and 1930. An adequate theory of the nature of the reaction of light has only become available in the last three or four years, and our views as to the nature of the development reaction have changed very radically in the last year or two.

About twenty years ago, an attack was made on the origin of the sensitivity of the silver bromide crystals by three groups of investigators—Svedberg and his collaborators in Sweden, the British Photographic Research Association in England and the Kodak laboratories in Rochester, N.Y. Soon after gelatin emulsions were first made, it was found that the exposure required in the camera was greatly lessened if the emulsion had been cooked for some time at a high temperature or if it had been treated with ammonia. It was observed that the grains had grown larger during this treatment, and it was concluded that their greater sensitivity was due to their greater size. This is true, but it is by no means the whole story. For a long time, it had been known that if an emulsion were treated with some chemicals, such as chromic acid, it lost its sensitivity, even though the size of the grains was not changed. Also, sensitivity depends very much upon the particular kind of gelatin used in making the emulsion. No clue to an explanation was found until Dr. S. E. Sheppard studied systematically the various fractions obtained at each stage in the preparation of photographic gelatin.

Photographic gelatin is made from clippings from the skins of calves. For this purpose, the skin of the face and ears is used because these parts are of no value for leather. These clippings are first washed and then treated with lime for a long time to remove the fat and hair. The lime is removed by long washing with weak acid and then with water. Then the material is cooked in steam kettles until the gelatin is extracted, and the extract is concentrated if necessary and allowed to set to a jelly ; the blocks of jelly are cut into thin slices and stretched out on nets to dry. Sheppard found that in the acid liquors in which the limed clippings had been washed there seemed to be a concentration of some sort of sensitizer, eventually identified as mustard oil, which contains sulphur.

When mustard oil is treated with alkali, it forms allyl thiocarbamide. If silver bromide is treated with a solution of allyl thiocarbamide, the surface of the silver bromide is attacked and grows a mass of white needles containing both allyl thiocarbamide and silver bromide. When these are treated with alkali, they break down into little black spots which must consist of silver sulphide because of the chemistry of the reaction.

When the development of grains is observed under the microscope, it starts from specks, these increasing in number and size until each grain is transformed into metallic silver. The question arises as to whether these centres of development existed before exposure or came into existence when development started. Sheppard's work put the whole matter beyond doubt. Sensitivity depends upon the existence of specks far too small to be seen in the microscope, and these specks consist of silver sulphide, probably derived from the mustard oil in the gelatin. The amount of

* Abridged from *Scientific Monthly* (Lancaster, Pa.), **55**, No. 4, 293–300 (Oct. 4, 1942).

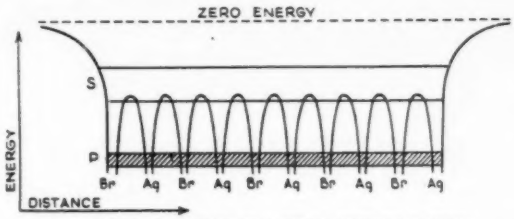

Fig. 1. ENERGY DIAGRAM OF A SILVER BROMIDE CRYSTAL.

mustard oil in an emulsion is very small. A ton of emulsion contains only a couple of drops, and the evidence for the existence of the sulphide specks is therefore indirect.

When Sheppard found that the sensitizing specks consist of silver sulphide, he and his colleagues advanced a theory of the action of light which they called the 'concentration speck theory'. The silver sulphide specks, they suggested, are formed on the surface of the silver bromide crystal and must in some way enter into the lattice of atoms of which the crystal is composed. They produce strains in the crystal, therefore, and these strains stretch into the surface of the crystal as an area of weakness. When light falls on such a crystal, energy travels through the crystal until it reaches the boundary of the speck. At this boundary, owing to the sudden change in structure, metallic silver is set free from the silver bromide. The sensitizing speck thus acts as a nucleus for collecting or concentrating the energy throughout the whole area of the crystal and for liberating metallic silver at the speck itself.

There was still wanting a mechanism for the operation of this concentration speck theory. Such a mechanism has been supplied by the work of Dr. R. W. Gurney and Prof. N. F. Mott and of Dr. J. H. Webb. In the first place, if we consider the energy diagram of a silver bromide crystal as a whole (Fig. 1), there are two energy levels of importance in the photographic process, the *S* and *P* levels, in which the electrons may be situated. The *S* band is normally empty and is referred to as a 'conduction band'. The *P* band is normally completely filled with electrons. Upon exposure of a silver bromide crystal to light which is absorbed in the long wave-length end of the characteristic absorption band, the electrons are transferred from the lower *P* band to the *S* band, and the crystal becomes conducting. This property is well known in other materials, as well as in silver bromide, as 'photo-conductance', and the silver bromide crystal exposed to light may be imagined to be filled with a sort of gas of conducting electrons. This is the primary photographic process—the thing that happens instantly when light falls on the crystal (Fig. 2). When the electrons set free in this way reach a sensitivity speck, either consisting of an impurity or of the silver sulphide derived from the gelatin, the electrons will be trapped and the sensitivity speck will become negatively charged by the electrons that it has absorbed.

In a crystal there are always mobile silver ions in the silver bromide lattice, and these silver ions will travel through the crystal until they reach a sensitivity speck, and there the charge on the ion will be neutralized by the negative charge on a trapped electron and the silver atom freed by the discharge of the ion will be deposited at the sensitivity speck, so that the electrons set free into the conductivity level by the original exposure to light are eventually transformed into silver atoms deposited on a sensitivity speck. This theory postulates that the production of an image by the action of light occurs in two stages : first, the release of free electrons which occurs instantaneously, and then the transfer of the free electrons by neutralization through the silver ions into silver atoms at the sensitivity specks.

This theory of Gurney and Mott supplies a mechanism for the phenomenon known in photography as the 'failure of the reciprocity law'. Bunsen and Roscoe found that, for the photochemical production of hydrochloric acid, time and intensity were reciprocal. That is, the production of hydrochloric acid was independent of the rate at which light energy was supplied and was dependent only on the total amount of energy. In the case of photography, however, it is well known that the effect of light is not independent of the rate at which it is supplied. There is an optimum rate for the formation of the maximum amount of photographic image, a rate corresponding to an exposure of a small fraction of a second. If the exposure is much greater than this—an hour, for example—two or three times more energy may be required to produce the same density of deposit. On the other hand, if the exposure is much shorter—a ten-thousandth or hundred-thousandth of a second, for example—somewhat more energy will be required. In order to account for the inefficiency of very low intensities of light in producing an image, it must be assumed that the silver atoms formed at the sensitivity specks can easily be lost in the early stages of the formation of the image by acquiring a positive charge owing to thermal action. Thus, for example, newly liberated silver atoms would be more easily lost than those which have entered a silver crystal lattice to form metallic silver.

On the other hand, with high intensities of light the electrons are supplied too rapidly, the silver ions do not have time to reach the sensitivity specks by diffusion through the crystal, and some of the electrons are repelled from the highly charged sensitivity specks before they can be neutralized. Much light has been thrown on the nature of the reciprocity failure by a study of the formation of the image at different temperatures.

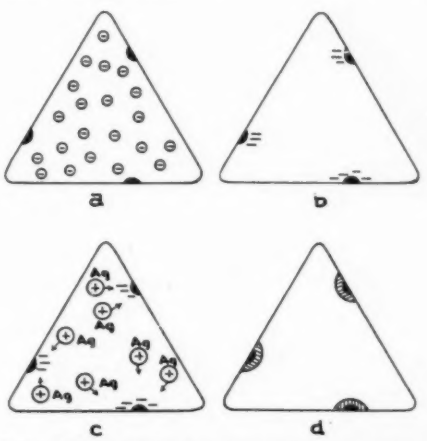

Fig. 2. ACTION OF LIGHT UPON A SILVER BROMIDE CRYSTAL. (*a*) LIBERATION OF ELECTRONS INTO THE CONDUCTING LEVEL. (*b*) CHARGING OF THE SENSITIVITY SPECKS BY THE LIBERATED ELECTRONS. (*c*) MIGRATION OF SILVER IONS TO THE CHARGED SPECKS. (*d*) DEPOSITION OF SILVER ON THE SPECKS TO FORM THE LATENT IMAGE.

FIG. 3. ELECTRON MICROGRAPH OF A GRAIN OF DEVELOPED SILVER. ×50,000.

The metallic silver deposited at the sensitivity speck and forming a nucleus which facilitates development is what has been known in photography from the earliest days as the 'latent image'. There has been much discussion as to the nature of the latent image, but there is now no question that it consists of metallic silver concentrated at local points in the silver bromide crystal. If the silver bromide is exposed to large amounts of light, a visible discoloration takes place, and this undoubtedly consists of metallic silver. This can be shown by the use of the X-rays in the ordinary crystal structure analysis, when characteristic rings of silver crystals are obtained, and also by the examination of the image by means of the electron microscope, when it can be seen to consist of crystals of silver. Inasmuch as the visible image produced by long exposure to light consists certainly of metallic silver, the latent image produced by short exposure is almost certainly of the same nature.

The mechanism of photographic development has always been very obscure. The general theories as to the nature of development until quite recently were those which conceived it as being similar to crystallization from a supersaturated solution of metallic silver. In the absence of nuclei, deposition of silver would be delayed, and this would check further solution of the silver bromide. In the presence of a latent image nucleus, deposition of silver is facilitated and the solution, reduction and deposition of the silver bromide continues until the crystal is converted into metallic silver. There are many physico-chemical difficulties in this mechanism, but it would undoubtedly have continued as a possible explanation of development had it not been for the evidence recently supplied by the electron microscope as to the form of the silver produced in development.

The silver grains of the developed image are very small and the microscope is unable to distinguish any appreciable structure in them, so that in spite of some suggestions that the silver might be formed as filaments, the general view was that the developed silver grain was a spongy mass of silver crystals somewhat resembling a lump of coke. When developed silver was examined with the electron microscope, however, it was at once seen that the silver grain is a tangled mass of ribbon-like filaments looking very much like a mass of seaweed (Fig. 3). This is not only very important for the light that it throws on the actual structure of the developed image, but also it at once becomes necessary to account for the production of such an unusual form of silver in the development of the silver bromide. Clearly, the supersaturation theory of development will not do at all. It might be thought that the filaments are formed in cracks in the crystal, but this was disposed of by an examination of the very small crystals of so-called 'Lippmann emulsions', crystals too small to be seen in a microscope. With the electron microscope, these appear as normal silver bromide crystals, but when they are developed, each of them turns into a single filament of silver, these filaments being much longer than the diameter of the crystal from which they were formed. The impression, therefore, becomes overwhelming that the silver filament is formed by ejection from the silver bromide crystal. It is squirted out during the process of development from the solid crystal, and the picture we form of the development reaction is that the reducing ions of the developing agent attack the silver bromide crystal, and in some way the initiation of the formation of metallic silver is facilitated by the presence of the latent image speck.

The mechanism for this is supplied if we consider the potential at the surface of the silver bromide. The surface of the silver bromide grain itself has an excess of bromide ions, which give rise to a negatively charged surface. However, just outside this negative charge, a positive layer of potassium ions must be present to neutralize the negative charge. Without such a neutralizing layer of positive ions, it would be impossible for the surface of the silver bromide grain to be covered with negative bromide ions, since the amount of such a charge in so small a region would give rise to explosive forces. A double-charge layer, consisting of negative bromine ions on the grain and positive potassium ions in the gelatin just outside, may be considered to exist around the surface of each silver bromide grain (Fig. 4). Grains with such a double layer (in a solution) would move under an electric field as negatively charged bodies, since the negatively charged grain would be forced in one direction by the field, and the surrounding movable positive-ion layer in the opposite direction ; but since at any point in the liquid there would be positive ions to form the surrounding positive shell, the

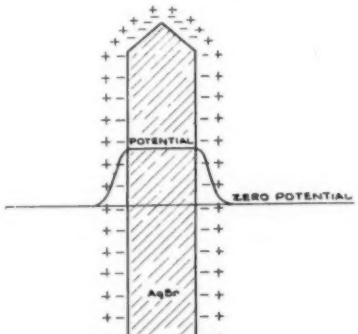

FIG. 4. DISTRIBUTION DIAGRAM OF THE POTENTIAL AND CHARGE OF A SILVER BROMIDE CRYSTAL.

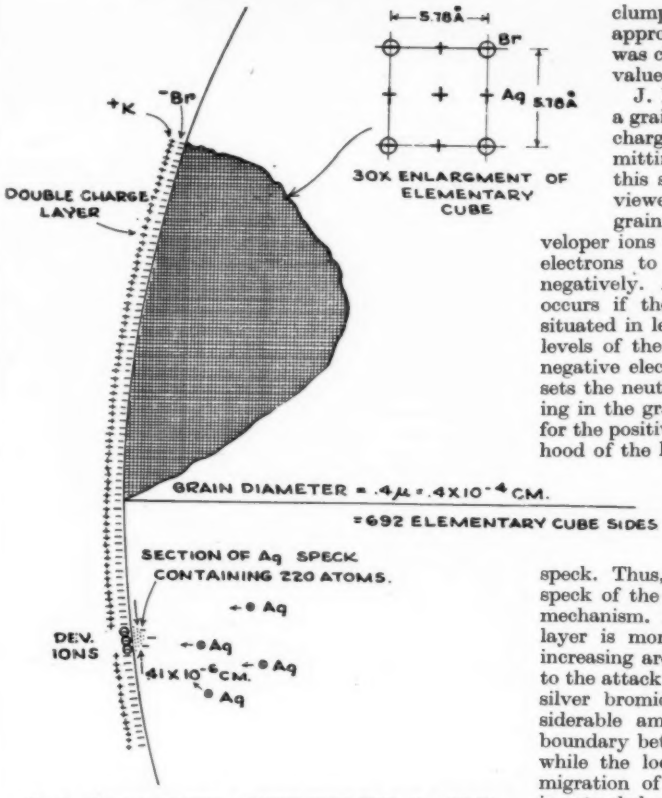

Fig. 5. DIAGRAM SHOWING A LATENT IMAGE SPECK IN A SILVER BROMIDE CRYSTAL ATTACKED BY A DEVELOPER.

double-charge layer would be maintained. That the surface charges on the particles and surrounding charge layers do neutralize each other in the manner outlined is proved by the fact that the colloidal suspension does not possess a net charge of either sign but is neutral as a whole.

It may be assumed that a grain, owing to its double-charge layer, behaves toward outside charges and also those located inside the grain as a neutral body. An electron placed inside such a double-charge layer would experience no force nor, in the same way, would an electron placed outside such a double layer. However, there is a marked difference in potential between the inside and outside of the grain, and the total jump in this potential occurs in the region between the two charge layers. The potential gradient between these charge layers accordingly gives rise to a strong electrical force between the layers, and an electron placed between them would experience a force toward the outside. It is considered that the double-charge layer acts in this way as an effective potential barrier to the entrance of an electron into the silver bromide grain of the emulsion and prevents the developer from attacking the grain.

The conditions existing in the exposed grain containing a latent image silver speck may be seen in Fig. 5. This shows a greatly enlarged scale model of a charged grain surface with a clump of silver atoms on the surface, which is supposed to represent the latent image produced by exposure to light. The clump shown includes 220 atoms, with approximately the correct spacing. This size was chosen as representing a fair mean of the values given by various workers.

J. H. Webb assumes that development of a grain is initiated by the break in the double-charge layer caused by the silver speck, permitting the negative developer ions to reach this silver speck. The latent image speck is viewed as an electrode penetrating into the grain. The tendency on the part of the developer ions to release electrons to the silver causes electrons to pass to the electrode and charge it negatively. As explained by Gurney and Mott, this occurs if the electrons of the developer ions are situated in levels above the highest occupied energy levels of the silver metal. The penetration of this negative electrode into the silver bromide grain upsets the neutral electrical condition previously existing in the grain, and there arises an attractive force for the positively charged silver ions in the neighbourhood of the latent-image speck.

Positive silver ions always exist in the crystal lattice owing to temperature motion, and these diffuse to the speck under the attraction of the negative charge there and enlarge the silver speck. Thus, it is supposed that the original silver speck of the latent image continues to grow by this mechanism. As this proceeds, the protective double layer is more and more ruptured, and a rapidly increasing area of the silver halide grain is exposed to the attack of the developer. Since the reduction of silver bromide by a developer is exothermic, considerable amounts of energy are liberated at the boundary between the silver and the silver bromide, while the local rise in temperature facilitates the migration of silver ions, so that the metallic silver is extruded as a filament.

The process of development itself involves certain requirements as to absorption and catalysis of the development reaction, which explain why one substance is a developer and another reducing agent having approximately the same reduction potential is not a developer. Much work remains to be done on the chemical reactions involved in development, but the knowledge which has recently become available as to the formation and nature of the latent image and its function in promoting the development of the silver halide grain has enabled us to form a much clearer picture of the phenomena of the photographic process than was possible previously.

EFFICIENCY AND OUTPUT IN AGRICULTURAL SYSTEMS*

By PROF. A. W. ASHBY

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THE measurement of outputs of agricultural systems and their valuation are always a complex proceeding, and the results of any methods of measurement and valuation which have been used up to the present time are to some extent indefinite. Outputs vary in their physical and biochemical characteristics, and in their human uses and values. The only common measure of outputs of varied characters is their

* Excerpts from a paper read before the Royal Society of Arts on November 11.

money value at any given time. Relative prices of one product and another vary from time to time, and therefore the values of outputs of given quantities of products also vary. A system or type of farming which may be shown to be efficient at one period may prove to be relatively inefficient a short time later, due solely to changes which have occurred in markets.

In any economic and social system which is subject to major changes, adaptability to changes in markets for produce, in industrial production or commercial offers of the raw materials used and usable in agriculture, in industrial production of agricultural equipment, in supply and costs of human labour, must always be a test of farming efficiency. It may, indeed, be the acid test from which there is no hope of anything except a very temporary escape. There is scarcely any major change in the economic and social institutions and conditions of a nation which does not, sooner or later, affect forms of organization for agricultural production and their results.

A quarter of a century ago many essays were made to judge agricultural systems on their outputs of food values—in carbohydrates, fats, and proteins—in the common measure of calories. But modern knowledge of nutrition, by adding vitamins and minerals to the elements expected in food supplies, has severely qualified these tests. There is no common measure which can be applied to all the food values of the products of any two or more agricultural systems.

But even when we thought we were able to measure and value agricultural outputs in physical and biochemical terms, we were quite unable to measure inputs in these terms, and therefore were unable to judge the efficiencies of systems of production. The 'calorie' test was usually only quoted per acre of land, which itself is more a measure of superficial area than of productive capacity, and any other test was rare. A simple illustration of this test shows its weakness.

The order of yields of calories and of protein per acre, of the following crops, in human use of the main products and of the secondary products of their by-products is :

	Calories per acre (order)	Protein per acre (order)
Sugar Beet	1	3
Potatoes	2	1
Wheat	3	2

While wheat and pig-meat produced by the offals give less than 45 per cent of the yield of calories obtained from sugar in sugar-beet and milk from tops, they give nearly three times the protein yield. But when the test of production per man-hour of labour is applied the order is entirely changed :

	Calories per man-hour	Protein per man-hour
Sugar Beet	3	3
Potatoes	2	2
Wheat	1	1

Wheat gives much the largest return of energy per unit of manual labour—nearly three times the return of protein when compared with potatoes, and about eight times the return of protein when compared with sugar-beet.

We may, indeed we must, occasionally apply the physical and biochemical measures to agricultural outputs in relation to human food requirements or supplies. But they are only crude tests of efficiency in feeding the nation. They are not tests of efficiency in production.

Every nation at any time, and particularly in war-time, must seek to make the optimum use of each and all its resources ; and the maximum use of land, maximum production per acre, tends to waste human energy everywhere. It nearly always wastes sweat, and not uncommonly blood and tears. A nation which has a very dense population, or a dense population tending to increase, and few resources other than its surface soil, may have no option but to seek maximum use of land, and highest possible production, but such a nation is to be pitied rather than envied. Or, in these days of close international relationships, we should say : pitied and assisted to find other outlets or uses for labour.

The general principle is that each nation will determine its standard of production per acre of land in relation to two main conditions : (1) the existence of other resources or means of useful employment of population ; (2) the standard of remuneration and of living established for workers in industries other than agriculture. When full examination is made of the economic and social conditions of nations with high density of agricultural population and high production per acre, it will be found that they adopt these conditions of necessity rather than choice. If they had other resources on which to use labour, they would modify their agricultural systems.

Efficiency, whether of motor-cars, of cows, or of crop-acres, is always measured by the ratio of output to inputs. Outputs are the results of inputs, with some factors, such as weather, which may be beyond the producer's control or even influence. Inputs are determined by the technical knowledge and the capital resources of the farmer and according to his judgment of the money costs and the money values of the expected result in outputs. Every single input may be applied with different degrees of economy and efficiency.

The main inputs are land, human labour—manual and managerial—and capital. But capital takes many concrete forms—in live and dead farming stock and materials. If we wish to apply even simple tests of efficiency of farming systems we must test production (*a*) per unit of land, (*b*) per unit of labour, (*c*) per unit of capital. We must also test by the best combination of measures of these inputs that we can find. For this purpose each has to be valued and measured in money.

The productive capacity of land or farms in Great Britain is far better measured by money values as rentals than by superficial area. The productive capacity of two acres in different districts may vary as much as 100 : 1 and commonly varies as much as 25 : 1. Rental values tend to represent and to equalize measures of productive capacity. There is no other common measure of all the geophysical conditions which influence the productive capacities of different units of land as such.

The technical conditions of choice and application of some of the embodiments of capital, both fixed and circulating, are too varied for general discussion. But some conditions may be indicated. The efficiency of power sources and of implements and machinery has been rising. It appears that the input of feed required to produce a gallon of milk has fallen by about one sixth in this century because of selection of breeds, strains, and individual animals, and greater knowledge and better practices in feeding. The feed required to produce a given weight of mutton or lamb, and the time and probably the feed required to produce a 10-cwt. fat bullock, have also been reduced.

The amount of feed required to produce a dozen eggs has been reduced by some 12–15 per cent. There is also evidence of rising efficiency in crop production.

The best simple measure of the general rise of efficiency is found in the output per man, which has probably risen about 70 per cent in the last seventy years. From 1925 to 1939 the increase was very rapid. Different criteria of efficiency may be applied by interested parties. If agriculturists as a community have to apply any single standard to production they will judge it by the net output per man (gross output less cost of raw materials and depreciation of equipment), for it is this which determines the possibilities of their standards of remuneration and of living. Having set a high standard of output per man, each class can bargain for its distribution with the assistance of any social forces they can draw to their aid. But with a low standard of output the standard of remuneration of one or more of the classes, and possibly all, must be low.

The general practice seems to have been to make international comparisons mainly in terms of yields or outputs per acre. But recently there has been a change, and other tests have been applied. On the information now available, it appears that Great Britain will take different positions in an international order of productivity according to the test applied. In a list of twelve countries—New Zealand, Australia, Great Britain, U.S.A., Denmark, Canada, The Netherlands, Belgium, Switzerland, France, Germany, Eire—the position of Great Britain is : (1) value of output per acre, seventh ; (2) persons fed per acre (on cash value basis), sixth ; (3) persons fed per worker, third (excelled only by Australia and New Zealand) ; (4) value of output per worker, third (again excelled only by New Zealand and Australia). In a test with seven European countries—Great Britain, Germany, France, Denmark, Belgium, The Netherlands, and Switzerland—the position of Great Britain is : (1) value of gross and net output per acre, seventh ; (2) value of output per worker, first ; (3) average income per whole-time person engaged, almost certainly first.

But in case there is a tendency still to judge yields or outputs per unit of superficial area of land, that is, per acre, it may be pointed out that among the seven European countries, Great Britain is second only to Denmark in gross output per pound sterling annual (rental) values, and second only to France on net output per pound sterling annual (rental) values when a standard rate of interest is applied to capital values of land in each country. The rate of interest on capital values represented by gross rents or prevailing rates of interest on mortgages is higher in Great Britain than in any of the European countries listed. Measuring gross and net outputs against annual values of land as determined by the prevailing interest rate in each country, which varies between about 2·2 per cent in Belgium and about 5 per cent in Great Britain, this country stands fourth in the list.

If Great Britain wishes to retain an agricultural population, which will be satisfied and satisfactory from either economic or social points of view, it will continue to be necessary to maintain a high standard of net output per worker, and all other standards have to be fitted to this condition. Even in war-time we have to make the optimum use of labour ; we cannot afford to waste labour on unproductive land ; we must use our labour where it will be most productive of the supplies of which the nation has the greatest need.

The nation will judge by the quantities and qualities of products offered for human use and consumption in relation to its requirements, normally expressed through the markets, but in war-time expressed through the administrative organs of the State (Ministries of Agriculture, Food and Supply). Beyond this the nation has never had any clear criteria. The interested but poorly informed public has many and often conflicting criteria. It wants reasonably cheap food, maximum employment of labour, maximum production from the land, more small holdings, higher if not maximum wages and better housing for farm workers. It wants more wheat for national defence, more milk and vegetables for health, more eggs and more and higher quality meats to stimulate appetites and to meet needs due to changes in industrial work, urban living, and standards of real incomes.

Specific tests are applicable for special purposes in certain circumstances. The agricultural system of each country is more or less fitted to its general economic and social conditions. The conditions established in one country are not necessarily to be sought by any other, and the standards applied in one country are not likely to have any real validity in any other, unless the general economic and social conditions are similar in both.

Perhaps the most acute controversy about standards of production and efficiency in the future will turn on the efficiency of large farms. Past studies of the smaller and larger farms in Great Britain have usually shown that the smaller farms had the higher outputs per acre and the lower outputs per man. But in any discussion of this problem it must be remembered that few of the large agricultural enterprises in the country have been organized on unitary holdings, and also that there is no public information on the details of organization, inputs and outputs of the best of the large enterprises in Great Britain. Before any firm judgment can be made on the relative economic and social results of the largest, the medium and the small enterprises, we need much more information on the largest enterprises. At present information is practically limited to the small and medium-sized businesses.

A further caveat must also be entered. Even if it were proved that the results of the larger enterprises now in existence are not any better than those of the medium-sized or other groups, that would not prove that large unitary enterprises could not be designed, organized and operated to yield highly satisfactory results.

GEOLOGICAL MAKING OF INDIA

MR. D. N. WADIA, author of the well-known "Geology of India for Students", retired from the Geological Survey of India in 1938, and shortly after took up the appointment of mineralogist to the Government of Ceylon. He was president of the Geological Section of the Indian Science Congress at its jubilee session in Calcutta in 1938, and became general president of the whole Congress at the twenty-ninth session held this last January at Baroda ; he is the third geologist to fill this position. During his Indian service Mr. Wadia was engaged mainly in the study and mapping of the post-Archæan folded formations of north-western India,

including Kashmir. In his present service, in Ceylon, he has been called upon to study the rocks at the other end of the geological scale, forming the Archæan basement. This exceptional diversity of opportunity has enabled him in his presidential address to the Indian Science Congress (Proc. 29th Ind. Sci. Cong., Part II, 3–23; 1942) to discuss with wide experience the "Making of India", in which he makes particular use of data derived from Ceylon, which, of course, is only a detached fragment of the Peninsula of India.

Mr. Wadia emphasizes the well-known general distinction between (1) the geologically speaking, recently folded extra-peninsular area of India, comprising the mountain festoons of Baluchistan and the North-West Frontier on the west, of the Himalaya to the north, and of the Assam-Burma ranges to the east; and (2) the geologically ancient and stable Peninsula, with its Archæan basement, superposed Purana and Gondwana sediments, and Deccan Trap basalt flows, and the absence of any serious folding movements since pre-Cambrian times. Such mountains as now exist in the Peninsula of India are either deeply eroded remnants of pre-Cambrian ranges, such as the Aravallis and the Eastern Ghats, probably (in the reviewer's opinion) re-elevated, or are plateau mountains due to denudation, with block uplift and depression, as in the Deccan Trap section of the Western Ghats.

In the extra-peninsular area, compressive earth forces still prevail, as instanced by the frequency of serious earthquakes; while in the Peninsula tensional forces with vertical uplift and depression have long prevailed, and serious earthquakes no longer occur. This essential difference between the extra-peninsular and the peninsular areas of India was noticed by W. T. Blanford, in the first edition of the "Manual of the Geology of India", so long ago as 1879, and was recently emphasized by myself (*Rec. Geol. Sur. India*, **62**, 408; 1930). As Mr. Wadia points out, this fundamental difference between the two main portions of the Indian sub-continent is due to the fact that it is composed of two crust-blocks of totally different history, namely, the Archæan shield of the Peninsula, which is a fragment of Gondwanaland, and the folded belt of the Himalaya representing the Tethys, which by their interaction have produced the Indo-Gangetic plains.

In Ceylon, the whole island is occupied by an Archæan crystalline terrane, except for Miocene rocks occurring mainly as a fringe on the north-western edge, and a minute occurrence of Jurassic rocks. The central portion of the island is mountainous, rising to 8,292 ft. in Pedrotallagalla. Three well-marked terraces or peneplains have been described by Prof. F. D. Adams, and are recognized by Mr. Wadia. "These three terraces rise from the sea in two sharply cut steps or escarpments, the lower step about 1,000 feet above the coastal peneplain (which is very nearly a plain of marine denudation) and the second step 3,000–4,000 feet above the latter to the third and highest peneplain" (p. 14). These escarpments are characterized by numerous waterfalls.

There are two possible explanations of these terraces. They may represent successive levels of denudation, in which case the lowest terrace is the youngest, and all that is required to explain this peculiarity in the orography of Ceylon is successive uplifts of the island as a whole, the mural scarps being due to the effects of differential erosion on beds of different powers of resistance. Or the terraces may be due to selective block uplift, and the mural scarps represent eroded fault scarps. The coastal terrace is then the oldest, and the mountain centre of the island the region of latest uplift. Adams, in his study of the geology of Ceylon (*Canadian J. Research*, **1**, 425–465, 467–511; 1929), adopts the former explanation, and Mr. J. S. Coates, who was Mr. Wadia's predecessor as Government mineralogist, does not disagree with Adams in his "Geology of Ceylon" (*Ceylon J. Sci.*, **19**, Sect. B, Part 2, 101–186; 1935); nor does Coates's geological map, which is admittedly only a sketch map, show any necessity for block faulting. Mr. Wadia, in his address, discusses only the second possibility and states: "The field geological work so far carried out in this area indicates, beyond doubt, that the Ceylon mountains are not the undenuded passive remnants left out of an old table-land, but have been 'created' by positive earth-movements, lifting them vertically in two intermittent, widely separated stages". In his view the second peneplain was elevated in post-Jurassic times, and the uppermost and central terrace in late Tertiary times.

In the absence of the evidence upon which Mr. Wadia bases his belief in selective block faulting, it is difficult for the reader to judge between these two possibilities. Adams's explanation seems so reasonable that the reader will probably prefer to follow him until Mr. Wadia produces his evidence, which he has deferred to an occasion more suitable than an address to a general audience. Should Mr. Wadia's detailed account carry conviction, then the credit for establishing this hypothesis of selective block-uplift to Ceylon will belong to him.

In his general discussion of the making of India, Mr. Wadia raises many points upon which there is scope for difference of opinion; lack of space prevents reference to them all. It is necessary, however, to demur to two passages in particular, namely, that (p. 9) which speaks of the "unpuckered and generally horizontally bedded rocks" of the peninsular crust-block of India, and the statement that Mount Everest is built of "this axial granite core". Many of the Archæan gneisses and schists of the Peninsula are as contorted and crumpled as any in the Himalaya; while Mount Everest has been shown by Odell and Wager to be composed largely of metamorphosed sedimentary rocks.

In the first part of his address, devoted to general matters, Mr. Wadia discusses the interactions of science and industry in India, and also the charge against science of "helping with its inventions and discoveries man's lower instincts and lust for power, possession and aggrandizement". This misuse of science is, of course, not due to the scientific man as such, and to check such perversion Mr. Wadia states (p. 5) that "it is time the hierarchy of pure science asserted its patent rights on the common pool of strategic science and, backed by its 1½ centuries of resolute strivings for the betterment of mankind, claim a determining share in the governments of the world. An international directorate of scientists, containing a due proportion of economists, engineers and industrialists, will, by adopting the technique and temper of science, govern the countries of the world better than the chancellors, diplomats and politicians who for the past 5,000 years have failed to bring harmony in human relations, but have signally succeeded in making history one record of recurrent wars."

L. L. FERMOR.

MECHANISMS OF VISION

Visual Mechanisms

Edited by Prof. Heinrich Kluver. (Biological Symposia, Vol. 7.) Pp. viii+322. (Lancaster, Pa.: The Jacques Cattell Press, 1942.) n.p.

THIS volume is the outcome of a symposium on "Visual Mechanisms" organized in September 1941 for the celebrations of the fiftieth anniversary of the University of Chicago. It is in twelve chapters, each written by specialists engaged in research on particular aspects of vision and each dealing with the advances and outstanding problems in a special field. With such a composition it might well have become no more than a series of disconnected reviews, but the authors and editor have made it into something which can be read straight through with pleasure by anyone interested in vision. Careful arrangement of the subject-matter is partly responsible, for we are led by easy stages from the material to the mental, from the photochemical reactions of the retina to the problems of visual recognition and learning. Thus the first chapter is by Selig Hecht, written with his usual lucidity, on the number of quanta needed for visual sensation. The chemistry of the visual pigments is dealt with by A. C. Krause and George Wald, there is a middle section on the activities of the visual pathways as they are revealed by electrophysiological techniques, and this leads up to the discussions by Lashley and Klüver on the way in which visual stimuli can be supposed to determine behaviour.

The book is a record of rapid progress made in the last few years, mainly along lines opened up by new techniques of physics and chemistry. The lines of advance were often unexpected and some of the classical problems of vision are not much nearer solution. There is no chapter on colour vision, for example, and probably there would not be much excuse for one, although Hartline and Granit have added significant data. But to compensate there is a valuable discussion of the factors determining visual acuity by Marshall and Talbot, based on the new electro-physiological methods. One of the basic (but seldom discussed) problems of vision is how we see, sharply defined, a straight line the geometrical image of which subtends in width only a small fraction of a retinal receptor. Marshall and Talbot have mapped the visual cortex in monkeys and have found that for the fovea the pathway from each cone expands on its way to the striate area, giving probably a direct connexion with a hundred cortical cells apart from any spread due to cross connexions at different levels. Thus the cortical mosaic has a much finer grain than the retinal; and they show that for a variety of reasons it is the cortical mosaic and not the retinal which decides the acuity of vision, the retinal mosaic being discounted by eye movements as well as by diffraction, while the cortical contrasts are intensified in a way which restores the sharpness of outline. In another chapter Bartley considers brightness discrimination from the same angle. Neither of these contributions makes easy reading, but arguments about neurone systems must always suffer from the complexity of the events with which they deal. Both chapters are certainly worth study as evidence that the analysis of activity in the units of the nervous system has already given place to attempts at synthesis, to the formulation of principles governing the distribution of activity in large groups of interconnected units. In the brain the messages from sense organs are not projected on a blank surface, but on one with a constantly changing activity of its own. We can begin to see what sort of interactions can occur and how the whole field may influence what happens in a limited arrival area: in fact the gap between the nerve impulses of the physiologist and the total configurations of the psychologist seems no longer immeasurably wide.

The two psychologists whose chapters end the book have never treated the gap between physiological and psychological aspects as if it were too wide to be contemplated with decency. Lashley has been concerned with it throughout his work and has made it far less formidable by defining the main obstacles. In this volume he deals particularly with the problem of learning and of 'stimulus equivalence'. What sort of physiological mechanism can explain our recognition of the letter A whenever we meet it? What we recognize is not the stimulation of certain neurones which have been sensitized by repeated use, for the neurones will vary with the size of the letter and its position in the visual field. Lashley rejects various theories but thinks that reverberating circuits in the cortex and interference patterns offer a more hopeful prospect. We must wait and see: meanwhile Klüver's article shows the fallacies of assuming that animals must see things as we do, and shows the careful analysis of behaviour which is essential before we can argue from the monkey to man.

Klüver rightly stresses some of the dangers of the comparative method when we are dealing with the visual system as a whole, but there is no doubt of its value when we are concerned only with the receptor apparatus. The descriptions of the anatomy of the retina by Walls and Polyak illustrate this and so do the chapters on the chemistry of the visual pigments. Wald's deals particularly with the relation of rhodopsin to vitamin A (a relation which Krause treats with some reserve), and the zoologist will be specially interested in his account of porphyropsin, the pigment in the eyes of freshwater fish, related to A_2 as rhodopsin is related to A_1, and of the rhodopsin–A_1 system in molluscs and arthropods.

The volume is worthy of the notable occasion for which it was designed. Though the War may limit its circulation we are deeply indebted to all who have taken part in it, for they have given us a welcome reminder of some of the fundamental problems of science.

E. D. ADRIAN.

TEMPERATURE CONTROL

Temperature Control

By A. J. Ansley. Pp. viii+126+6 plates. (London: Chapman and Hall, Ltd., 1942.) 13*s*. 6*d*. net.

ANY regular reader of a journal devoted to physics, or to scientific instruments, or to chemistry or to industrial chemistry, will find several articles each year in which a thermostat or controlled-temperature bath is described. In the view of its author, each of these new designs is superior to any of the old ones, and in fact they usually incorporate improvements, and are usually particularly suited to the problem in hand. Given then this vast mass of literature, it is certain that an investigator confronted with the problem of building a thermostat

system for himself will find it difficult to make a choice among the various systems unless he can have guidance from someone more experienced.

It is to meet the needs of such people, as well as of general students, that Mr. Ansley has written this book on temperature control, and no doubt it will be helpful to many. It deals, rather shortly, with all the main types (expansion of liquids, expansion of solids, vapour-pressure type, and so on) and frequently gives practical hints of great value. Nevertheless, it cannot be unreservedly recommended. The theoretical background is often given in a confusing manner, almost as though the author did not understand it himself, and there is in many cases an apparently deliberate departure from any logical order. Thus, on the first page, we find a section devoted to "Control Unit Terms", the first of which is "The Differential". Instead of being told at once what this is, we are informed that it "is most conveniently provided by immersing the apparatus . . . in a bath . . .". The next term, "Hunting", is explained in a way that gives it a different meaning from that usually associated with the word, and goes on (still under the same heading) to "The points to be remembered in control design are: . . .". Again, on p. 5, there is an explanation of vapour-pressure instruments which seems to imply, wrongly, that the pressure of the vapour depends on the size of the bulb. The function of the cone in a sulphur bath is not protection of metal against attack by sulphur, but to act as a radiation shield. The hydrogen in a barretter is introduced on account of its thermal conductivity and not primarily to prevent oxidation.

In addition to these major slips, there are far too many minor ones. Constantan is consistently spelled 'constantin', we are advised to use leads of 2 sq. cm. cross-section in thermocouples, the boiling point of sulphur is given on p. 29 as 445·5° and on p. 10 (where it is called the melting point) as 444·6°, the true value being 444·6°. In the table of coefficients of expansion, it is surprising to find that invar, with a higher coefficient than iron or copper, has been selected as typical.

Finally, it would be unfair to scientific workers in general if we did not point out that the grammar and style are much worse than we expect or are satisfied with. In this respect, as with the last-mentioned error and with the placing of a diagram upside down, the author has been badly served by his proof readers. They should have noticed some of the many occasions where a subject fails to agree in number with its verb, and they should have pointed out, when the author wrote " . . . it is feasible to suggest that . . ." that it is *feasible* to suggest anything whatever. They could, too, have saved him from many clumsy sentences; for example, "This oxide not only increases the contact resistance, but renders renewal frequently or cleansing of the mercury essential. Various attempts have been made to minimize this defect by providing a protective film of oil, such as glycerine for the mercury, but after some period of use it will not be found to have improved the control".

No doubt the book has been written in difficult circumstances, and this explains many of the blemishes, but the fact remains that the book is not as good as we could have expected from this writer. However wholesome the diet, our attention is distracted from its goodness, and our appetite spoiled, by the unattractive way in which the dish is served.

J. H. AWBERY.

WEATHER STUDY IN THE UNITED STATES

Weather Elements

A Text in Elementary Meteorology. By Prof. Thomas A. Blair. (Prentice-Hall Geography Series.) Revised edition. Pp. xvii+401. (New York: Prentice-Hall, Inc., 1942.) 5 dollars.

JUDGING by the fact that this book has been reprinted five times since its first publication in April 1937, and has now been revised, it has proved very popular in the United States. The public interest in meteorology is probably much greater there than in Great Britain; for one thing, the atmospheric phenomena are often more violent, culminating in disastrous tornadoes, floods or droughts, and the more specialized agriculture is so organized that favourable or unfavourable weather may make the difference between wealth and comparative poverty to a large agricultural population. In England our famous 'samples' are more easily balanced out by the more diversified farming. Secondly, in recent years the rapid growth of civil aviation has given many people an interest, amateur or professional, in the weather. Finally, the average American appears to be more addicted to the accumulation of varied knowledge than his equivalent in Great Britain; he is more ready to spend money on relatively expensive books, but in consequence he prefers a book which can be absorbed quickly and does not call for prolonged and intensive study.

Blair's "Weather Elements" meets all these needs admirably. It is attractively got up; it is full of interesting information reduced to a few lucid paragraphs on each subject; there are plenty of illustrations including some excellent photographs, and it is very easy to read. The intelligent member of the public with little physics and no mathematics will find it ideal. For the serious meteorologist it is less satisfying, for though it is very good so far as it goes, it does not go nearly far enough.

One example will suffice. The chapter on "World Weather" contains, among others, sections on "Frequency curves" and "Correlation coefficients and regression equations". The first gives two frequency polygons, with values of the mean, median and mode, but no attempt is made to explain the two latter terms. The polygon of 65 January rainfalls at Cleveland by steps of 0·2 in. is highly irregular; an isolated maximum at 1·40–1·59 in. is called the 'mode', but the reader is not warned that the record is too short to give such minor details any real meaning. Again: "Mathematical considerations also enable us to determine how often the rainfall will fall below or exceed any given amount". It is left at that; standard deviation is not even mentioned. A correlation coefficient "is computed by a mathematical process which takes account of and compares the individual deviations from average of the two sets of data year by year". There is not an x or y in the book; nothing, in fact, to break the smooth, even flow of text.

From another point of view the book is definitely written for an American public, in that there are few references to methods or events in other continents. The methods of observation described differ in many important respects from those in use in Europe; the dry- and wet-bulb thermometers are whirled in a sling instead of being read in a screen, the rain-gauge is read with a dipping rod instead of with a measuring

glass, the sunshine recorder is electrical and the cloud searchlight becomes a "ceiling light". The only barometer described is the Fortin, but the description is very sketchy and the illustration useless; the vernier is mentioned, but not described, and there is no indication of its purpose. The U.S. Weather Bureau fog scale is given, but the usual international definition of a fog is not mentioned.

The author is at his best in describing the simple physical processes of weather, and these parts of the book should be of real value to school teachers, who are also provided with a number of ready-made test questions and with several pages of monthly means of temperature and rainfall. There are a few odd statements, such as "Warm and humid air, as in a fog", and that air in crowded rooms suffers from *low* humidity. The chapter on "Climate and Man" contains a good deal of interesting material not usually found in text-books of meteorology, and the last chapter gives an excellent account of the history and activities of the U.S. Weather Bureau, especially in their application to matters like the control of floods or forest fires and the combating of frost damage, which should interest all citizens of the country. There is an excellent classified bibliography of suggestions for further reading.

C. E. P. BROOKS.

FOOD AND PLANNING

Food and Planning

By Prof. J. R. Marrack. Pp. 285. (London: Victor Gollancz, Ltd., 1942.) 15*s*.

THE dictum that the scientific man should 'have no politics' is still held to-day by many men of science. It is interpreted variously: that scientific research should possess the worker, excluding all else from his mind; he should have his energies fully occupied in solving his academic problems, and the applications of the results are 'politics', and should be left to others; he would be biased in his approach to an academic problem by an active interest in the practical applications of its solution. It is true the research type of mind is rarely adapted to apply the results socially, unless the biological trend of the investigator's thought is in that direction. The acute struggle for existence of the War of 1914–18 and its post-war period led to governmental organization of the applications of science to everyday problems. The planned surveying of man and his environment increased our knowledge of *Homo sapiens*, and stimulated further investigations. Possibly the main development of science between the two wars is the realization that there is a natural and close interdigitation between the pure abstract problem and its application in industry or human life. The scientific man is not a mental eunuch, but rather a rational human being, whose work must be conducted objectively and fearlessly, and whose reasoning and experiments need to be uninfluenced by conditions of expediency or possible repercussions.

This close connexion between science and the social life of the world's population is possibly most appreciated in physiology, and particularly in regard to nutrition. Prof. J. R. Marrack in this book exemplifies the relation of the research worker to the applications in everyday life. He shows that not only have we knowledge of the nutritive needs of human beings, but also that the bulk of the world's population is by these same standards undernourished in varying degree. If we feel the welfare of human beings is important, then we can, by planning for that purpose, achieve adequate nourishment for everyone in all countries, as Sir John Orr emphasizes in the preface.

The first part of the book deals with the normal foodstuffs, their chemistry and function, and with the physiological requirements of the individual. This elementary physiology is dealt with in a manner different from that of the standard text-book. The emphasis is on the needs and response of man, rather than that of animals. The evidence is new, modern and recent, and the experimental illustrations are culled almost entirely from human experiments, surveys and international experiences. An excellent account is given of the pathological distortions, and of the problems of standards of food requirements.

The next part of the book treats of the applications of this knowledge. Beginning with details of actual peace-time consumption of food in different countries throughout the world, it follows with a clear statement of the evidence for malnutrition and impaired health arising thereby. Next there is described the food consumption in Britain and on the Continent during and after the War of 1914–18, the ill-effects arising, and the administrative steps taken to combat this bad health. The last three chapters deal with the present War, the rationing and the respective policies of Britain and Germany. Finally, Prof. Marrack plans for the future as well as for our present war-time state.

The book is long, and although comprising only 285 pages, it is in small print, and achieves fairly—and sometimes very—detailed accounts. It is written for the educated man with an elementary knowledge of general science, but is essentially non-technical and straightforward, very lucid and always understandable. It is a book that will be frequently utilized by anyone interested in the subject: it is strikingly full of new names, facts, experiments, references and views, but its utility is seriously impaired (quite apart from the war-time price of 15*s*. net) by the complete absence of a list of tables and diagrams, of an index or a bibliography. Different individuals will question different parts, the attribution of responsibility, the weighting of evidence, but this is largely a question of legitimate difference of opinion or inference.

Probably one of the most important contributions of the book is the large body of evidence—physiological, sociological, economic and political—which Prof. Marrack has collected as to the causes of defective food supplies. It is patent throughout that the stimulus of a humanitarian philosophy has incited the author to write this book and to initiate a plan which he feels will improve human welfare. But the one construction of this plan is based on an objective examination of the evidence. It is built up dispassionately and logically, because it is in the best interests of the world to have no obstacles left that prevent the fullest nutritive health. Prof. Marrack further maintains that now, while the War is being fought, is the time to begin planning. "The redeeming sanity of war is that its purpose is clear, and its methods are logically directed towards the achievement of that purpose—can we keep this sanity after the war, and use the working machinery to plan an ordered society in which the needs of the people are the prime object?" (p. 284).

Prof. Marrack regards the world's food supply as being inadequate, because it is left to an economic system dependent upon a profit motive for its effective working. The breakdown may be in purchasing power, retail price, costs of transport, production, or governmental financial policy. So long, however, as food supply is controlled by factors other than the nutritional needs of the people, so long will that supply be liable to shortages.

Before this War, 90 per cent of the world population was in some respect short of food. The essentials of food planning include increasing the production of food. The biggest obstacles to this in different parts of the world are the small size of farms (33 per cent of farms in Japan are a quarter of an acre in size), debt on the farmer (£40 per head of population in Rumania) and backward state of agriculture (primitive instruments and methods). Planning must include regulation of amounts and type and transport of foodstuffs, and finally increased purchasing power. To attain all this with efficiency, Prof. Marrack proposes an international supreme authority with power to plan on a world scale, using the fullest resources of the globe, and with executive powers to carry its plans to fruition, so that no obstacle shall impede any individual from attaining full nutritive health.

A. ST. G. HUGGETT.

SURFACE PHYSICS AND CHEMISTRY

The Physics and Chemistry of Surfaces

By Prof. Neil Kensington Adam. Third edition. Pp. xii+436. (London: Oxford University Press, 1941.) 22*s*. 6*d*. net.

MANY branches of science have had their seeds planted some forty years ago and in the intervening period have germinated, grown and flourished to such an extent that massive trees now stand on previously bare earth. The physics and chemistry of surfaces is in this category, and the historian follows this growth first by means of a monograph and then with a text-book in which edition follows edition. Prof. N. K. Adam is universally recognized as one of the historians of this subject, and we welcome his third edition of his well-known text-book.

This volume follows the lines of his second edition, in which a few sections have been rewritten and certain important advances have been inserted in an appendix. It must be admitted that this is not a very satisfactory method of presenting an integrated view of the whole subject. Indeed, the fundamental difficulty presented to the historian goes somewhat deeper, in that subjects soon outgrow the confines of a single text-book. It would indeed be preferable to cut once and for all the Gordian knot and divide the subject into discrete sections, for example, into fluid and solid surfaces respectively. The problems of each would indeed be confined, but with difficulty, into the confines of single separate volumes.

The present edition gives an excellent account of the present state of our knowledge of the liquid surface, in which field Prof. Adam himself is a recognized authority, but many advances have been made in the study of gas films on solid surfaces as well as in understanding the mechanism of the various heterogeneous catalytic processes, more particularly those involving hydrogen, such as exchange reactions, hydrogenation, double-bond migration and ring closure in hydrocarbons, which might well have been included in an authoritative text-book.

The space which would be available if these topics were transferred to a separate volume could readily be filled by extension of certain portions of the present volume. Thus on p. 35 some thirteen lines only are devoted to the examination of films by reflected light. This method of examination, admittedly a difficult one, is a powerful method of attack, and several interesting properties of films have been revealed which might well have been included.

The general characteristics of surface films of insoluble materials can be portrayed by means of the phase rule, and the thermodynamics of the equilibrium between phases and changes of state have been shown to follow the three-dimensional prototypes. In ordinary phase changes there is a sharp transition in which the properties change discontinuously, including the area and the entropy (resulting from the latent heat). We know in three-dimensional systems that there are cases in which the change in volume and entropy are not discontinuous but merely change very rapidly within a few degrees; this is revealed by a large specific heat in the neighbourhood, but no latent heat. Such changes as are exemplified by molecular rotation in crystals or order-disorder transformation in β brass are phase changes of the second order. Dervichian, in a lengthy series of investigations, recently showed that such changes were also exhibited by monolayers. More than one page in the appendix might have been devoted to this work.

On pp. 38–39 a general description of the method of presenting the results of measurement of the phase boundary potential at liquid/air surfaces is given, and a further development of the concept of phase boundary potentials at liquid/solid and air/solid interfaces is given in Chapter 8 and the appendix thereto. Gatty's analysis of the phase boundary potential at the liquid/air surface provides us with the clearest picture of the origin of surface potential and should find a place in the introductory chapter.

It is clear that our knowledge of the structure of monolayers of macromolecules, especially of the biologically important proteins, is by no means complete. The author adopts the view that protein films are coherent. Several French investigators, on the other hand, have claimed that the vapour state can be obtained and actually measured, providing us with a method of determining the molecular weight. This clearly requires reinvestigation.

Only two and a half pages are devoted to consideration of the chemical reactions in films and the formation of complexes by the process of film penetration. The literature on this subject is now fairly extensive, and in view of the many biological implications it may be regarded as one of the most important aspects of surface chemistry, fully meriting a chapter to itself.

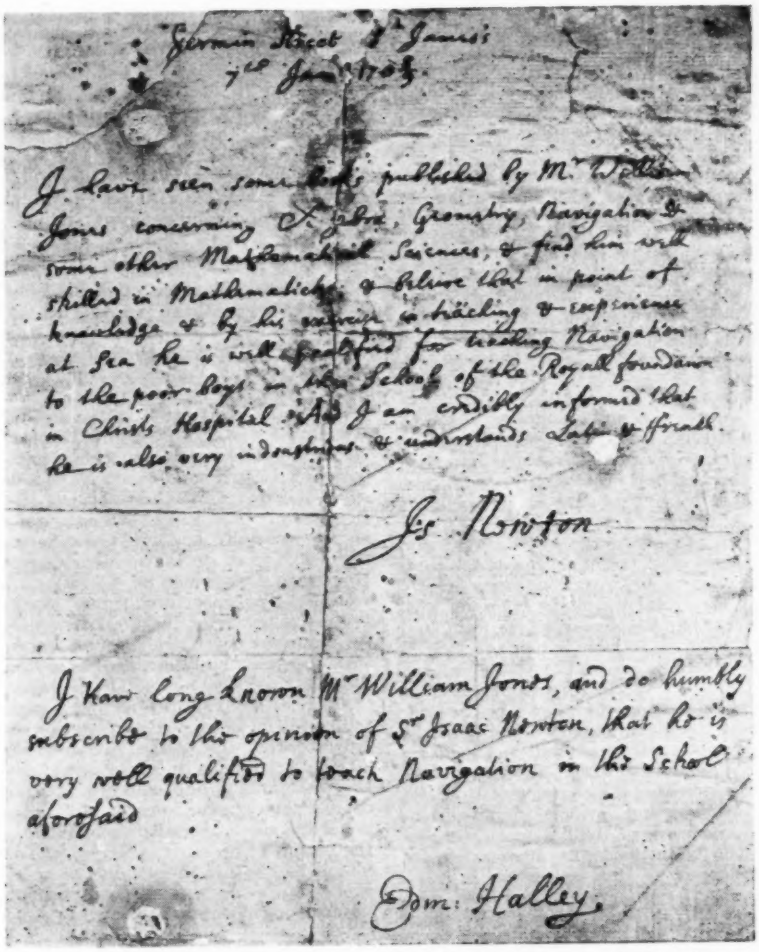
The third edition has been compiled with the author's usual care, and the topics given in the appendix have evidently been carefully selected with the view of presenting important as well as interesting developments in the field of the physical chemistry of surfaces. Scarcely any imprint of war is to be noted in the paper, binding, or printing of this, the third edition.

ERIC K. RIDEAL.

NEWS and VIEWS

Newton's Life and Works

AMONG the tercentenary celebrations of Newton's birthday was a meeting of the London and Home Counties Branch of the Institute of Physics held on December 9. The meeting was addressed by Dr. H. Buckley, of the National Physical Laboratory, and very appropriately was held in the Royal Institution, where a few days previously the Royal Society celebrations had taken place. Dr. Buckley's theme was "Some Historical Aspects of Newton's Work". He reviewed Newton's life in its relation to his scientific work, bringing out his interest in experimental studies and recounting the development of the papers presented, generally as the result of invitation, to the Royal Society. Speaking of the "Opticks", Dr. Buckley pointed out that, as in the "Principia", Newton develops the subject by the formal method of propositions, each followed by proofs, but whereas in the "Principia" the proofs are mathematical, in the "Opticks" they are accurate and clear descriptions of experiments. An informative account of the fate of the various editions of the "Principia" was given. As is well known, the first edition was produced at Halley's expense. It appeared in July 1687 as one small quarto volume of 500 pages bound in calf and was sold at 10*s*. to 12*s*. a copy. This sold quickly and by 1691 was difficult to obtain. A second edition appeared in 1713, produced with the assistance of Roger Cotes, the young Plumerian professor of astronomy at Cambridge; 750 copies were printed, of which 250 were sent to France and Holland. It sold in England at 15*s*. a copy, and up to 21*s*. bound in calf. Bentley, then master of Trinity College, Cambridge, bore the cost of publication


Jermin Street St James's
7th Jan. 1703
I have seen some books published by Mr William Jones concerning Arithmetic, Geometry, Navigation & some other Mathematicall Sciences, & find him well skilled in Mathematicks & believe that in point of knowledge & by his extraordinary teaching & experience at Sea he is well qualified for teaching Navigation to the poor Boys in the Schools of the Royall foundation in Christs Hospital. And I am credibly informed that he is also very industrious & understands Latin & French.
Js Newton
I have long known Mr William Jones, and do humbly subscribe to this opinion of Sr Isaac Newton, that he is very well qualified to teach Navigation in the School aforesaid
Edm: Halley

A TESTIMONIAL GIVEN BY NEWTON, AND SUBSCRIBED BY HALLEY, IN FAVOUR OF WILLIAM JONES, AFTERWARDS A FELLOW OF THE ROYAL SOCIETY, WHO WAS APPLYING FOR A POST ON THE STAFF OF CHRIST'S HOSPITAL. BY COURTESY OF THE ROYAL SOCIETY.

Cotes received twelve complimentary copies, but probably he refused all other recompense. A third edition appeared in 1726, but this contained only minor alterations; it was edited by Henry Pemberton, who received £200 from Newton for his work. Pemberton is also reputed to have received £3,000 for his "A View of Sir Isaac Newton's Philosophy", which was published by subscription; the subscribers numbered some 2,400, including peers of the realm, most of the bishops, and other notables including Alexander Pope and Edward Gibbon, a remarkable tribute to the esteem in which Newton was held. Collected editions of Newton's work were published in Lausanne and Geneva in 1744 in three volumes, and in London during 1779–85, though neither is complete. It is to be hoped that steps will be taken to produce a complete edition of the works of Britain's most distinguished natural philosopher.

Sir John Russell, F.R.S.

On September 30, 1943, Sir John Russell will retire from the directorship of the Rothamsted Experimental Station, which he has held since 1912, when he succeeded the late Sir Daniel Hall. During 1907–12 he was Goldsmiths' Company soil chemist at Rothamsted. In these thirty-five years of service Sir John has been associated with striking advances in agricultural science. An early achievement was the evolution of a method—then conveniently described as partial sterilization—for restoring the fertility of rich, but 'sick', soils such as tomato and cucumber beds. A sub-station of Rothamsted was established at Cheshunt, in the centre of the glass-house industry, to deal with this and related problems; its work was so successful that it quickly became an independent research institute. During the War of 1914–18 Sir John was technical adviser to the Food Production Department. Shortly after the War, the agricultural research and advisory organization was greatly expanded by the Government and, in common with other institutes, Rothamsted enlarged its staff and its programme of research to include plant pathology and entomology as well as its traditional subjects of soil science and crop growth. Important scientific advances were made in all of these branches and Sir John attacked the double problem of testing them under varying soil conditions and agricultural systems, and of accelerating their absorption into farm practice. He increased the contacts of Rothamsted with agriculturists and farmers' organizations until the objects and work of the Station became as well known in the countryside as among men of science. Striking proof of this was given in 1934, when the experimental fields were threatened with building developments: a public appeal was issued for funds to purchase the farm, and the amount required, £35,000, was over-subscribed in less than two months.

A further development, sponsored by Sir John, was the extension of Rothamsted's activities to overseas agricultural problems. This work began in 1923 when he was invited to the Sudan to advise on agricultural developments and scientific activities. Since that date he has made extensive visits to the Dominions, India and a number of the Colonies, as well as to many foreign countries, either by direct invitation from their Governments, or as the British Government delegate. He took a leading part in the Imperial Agricultural Research Conference of 1927, as a result of which the Imperial Agricultural Research Bureaux were set up, the one for Soil Science being located at Rothamsted under his direction. In pursuance of his policy of making widely known the scientific and practical results of agricultural research, Sir John has written a number of authoritative books on agricultural science and practice. The best known is "Soil Conditions and Plant Growth", which first appeared in 1912 and is now in its seventh edition. The successive editions contain their own record of the striking advances with which his name is associated. In recent years Sir John has done much to develop the international aspects of agricultural research. But for the War, Rothamsted would have celebrated its centenary in 1943 by an international gathering. This project has been submerged by the urgent war problems and the looming difficulties of post-war agricultural reconstruction in devastated Europe. In these problems Sir John is taking an active part. He serves as adviser to the Russian Branch of the Ministry of Information, and is a member of the Inter-Allied Government Committee on Post-War Requirements.

Bicentenary of Scheele (1742-1786)

The bicentenary of the birth of the Swedish chemist Carl Wilhelm Scheele, whose recently published manuscripts were reviewed in our columns on November 7, falls on December 19. His birthplace was Stralsund, in Pomerania, then a Swedish province, but he was trained as a pharmacist in Gothenburg. Here he remained until the age of twenty-three, studying the works of Stahl, Lémery, Kunckel and Neumann and for ever experimenting. From Gothenburg he went to Malmö, then to Stockholm and Uppsala, where he met Gahn and secured the friendship of Bergmann. At last in 1775, at the age of thirty-three, he was officially appointed apothecary at the little town of Köping on the western shore of Lake Malar. His death took place at Köping on May 19, 1786, when he was only forty-three. Among a crowd of original discoveries those relating to chlorine, manganese and oxygen are the best known. His papers were printed mainly in the *Transactions* of the Royal Academy of Stockholm. Dr. Beddoes translated his "Chemical Essays" in 1786 and a reprint of this was published in 1901. This was reviewed in Nature of November 7, 1901, the reviewer remarking then, "The strict fidelity to experiment, the rare sagacity, the scrupulous and minute observation and the extraordinary experimental skill combine to make Scheele a model for all time. When we add to this the pathos of his early struggles, the simplicity of his blameless life and the nobility of his untimely death, there can be no wonder that Scheele is reckoned a hero among chemists".

University Students and Military Service

In the discussions in the House of Commons on December 9 and 10 on the National Service Bill, 1942, Mr. E. Bevin, Minister of Labour, pointed out that the measure was largely an administrative one to facilitate the calling up of young men of eighteen. The position of prospective university students was discussed by several members of the House. Mr. K. Lindsay remarked that the Bill meant the end of university life in Great Britain during the War, except for a few specialists. Mr. Harvey asked for an assurance that if an undergraduate's work was satisfactory he would be allowed to complete an academic year before being called up. Sir P. Harris said that

any advance in education after the War would be impossible without a proper supply of teachers, and that the proposed call-up would paralyse the whole educational scheme of the country.

Mr. McCorquodale, Parliamentary secretary to the Ministry of Labour, replying to the debate, said that the medical schools would decide on the basis of the first examinations which students would be granted deferment. Scientific students would be recommended for deferment by the University Joint Recruiting Boards. The Minister of Labour had decided that he could no longer allow boys whose medical grade was sufficiently high to make them liable to serve to go up to the university to take an arts course for one year. This decision would entail the closing down at the end of the summer of the present year's university arts course with the exception of those who were medically unfit for the Services. As to the question of mathematics, if a scientific student persuaded the Recruiting Board that his course was in the interest of the prosecution of the War, he would get facilities for carrying it through. The Bill was read a third time.

Radio Research for the Services

MR. LYTTELTON, Minister of Production, said in the House of Commons on December 9, that in view of the great increase in the importance of wireless communication and radio-location during the War, the Government has set up a Radio Board as the co-ordinating body in regard to inter-Service policy, research, development and production. The Board is in general responsible to the Minister of Production, but where matters of service policy are concerned it takes its instruction from, and is responsible to, the Chiefs of Staff Committee. The Minister Resident for Supply in Washington (Colonel Llewellin) acted in his personal capacity as the first chairman of the Board. On his appointment to Washington he was succeeded by the Minister of Aircraft Production, Sir Stafford Cripps, also in his personal capacity. The chairman is assisted by two deputy chairmen, the Parliamentary Secretary to the Ministry of Supply (Mr. Garro-Jones) and Prof. G. P. Thomson. The membership of the Board includes representatives of the Admiralty, War Office, Air Ministry, Ministry of Supply, Ministry of Aircraft Production, and General Post Office, as well as several special non-departmental members. Much of the work of the Board is carried out through two main working committees, the Production Planning and Personnel Radio Committee and the Operations and Technical Radio Committee, of which Mr. Garro-Jones and Prof. Thomson are respectively chairmen.

New Light Day Bomber for the R.A.F.

RECENT reports of a day raid upon Eindhoven, Holland, mention for the first time the use of the Lockheed Ventura I. The R.A.F. type is presumably a development of the American machine, originally known as Lockheed Vega 27. This model is powered by two 2,000 h.p. Pratt and Whitney Wasp 18-cylinder radial engines, and has a speed of 275 miles per hour, with a range of 1,500 miles. It carries a crew of four or five, as necessary, and has armament of two movable and two fixed guns in the nose, a Boulton and Paul gun turret on the top of the body, and two movable guns underneath. It is probable that the improved version is superior to this in both performance and armament. The most significant point about the aircraft is that it is a further step towards completing a range of high-performance bombers and fighters that, in total, form a team for heavy day bombing, working upon the theory of smothering the defence when attacking. The Boeing Fortresses operate at great heights, Liberators at a little less, with Venturas, Mustangs and Mosquitoes lower down, even to ground-level. The high speeds and good manœuvrability of these machines make them able to take reasonable evasive action when attacked by anti-aircraft fire, their performance is comparable with British fighters, simplifying the question of their being escorted, and even unescorted their armament and manœuvrability allow them to put up a good defence against enemy attacking fighters.

Recruitment of the Civil Service

THE sixteenth report of the Select Committee on National Expenditure, in a section dealing with the recruitment of the Civil Service, includes some account of the Appointments Department of the Ministry of Labour and National Service, created on March 1, 1942, in which the old Central and Supplementary Registers have been merged, which should be of interest to scientific workers. As a recruiting agency the Ministry's organization now also includes the Temporary Government Service Registers and the Central Register of Aliens, and in regard to the latter, the report recommends that a much more vigorous attempt should be made to keep the Central Register up to date and to find suitable employment for the persons whose names it contains. Reviewing the operation of the Central (Scientific and Technical) Register, the report comments on the relatively slight use made of this Register by industrial firms as compared with Government departments. In regard to engineering appointments, the Committee recommends that academic qualifications should not be allowed to overshadow the less easily assessable but great practical value of men who have graduated through the workshops to management level. It is also recommended that to meet the shortage of persons of managerial capacity, the Ministry of Labour should make a careful search for persons who might be up-graded above the level of their present employment.

The importance is stressed of the Ministry preparing in good time for the difficult post-war task of placing in civil employment the large numbers of men and women demobilized from the Armed Forces, and attention is also directed to the importance, as a means of remedying the shortage of technically qualified recruits in Government departments—and the remark applies equally to industry—of ensuring the most economical use of such persons after appointment. This involves a careful watch on the changing conditions of industry from the point of view of the technical help and supervision needed from the Supply departments. The report also includes a memorandum by Sir James Rae on the professional and technical staffs in Government departments which describes the work of the assessors appointed early last year. This review covers the elimination of functions not essential to the prosecution of the War, the elimination, following if necessary a relaxation of normal peace-time standards, of all work now entrusted to professionally and technically qualified staff which is not absolutely necessary for the discharge of those functions, reduction to a minimum of checking and

reference to higher authority, increased collaboration between Departments with the view of avoiding overlapping of functions, a re-organization of the remaining work so as to make the minimum demands on professional qualifications, and to ensure that work not requiring such qualifications is devolved on persons less highly qualified. The report also recommends that delays in the payment of increases of salary either as increments or as the result of promotion should be eliminated, and the immediate investigation of the alleged inadequacy of salaries paid to technical officers. The Committee regards it as fundamental that there should be a reasonable relation between the salaries paid by Government and industrial firms respectively.

Agriculture in Northern Ireland

ADDRESSING a meeting at Ballyclare, Northern Ireland, Mr. J. F. Gordon, the Northern Ireland Minister of Labour, said that during the present year the farmers of Northern Ireland have surpassed all previous records in the production of food. They have increased land cultivation by 100 per cent, and obtained an agricultural turnover during the year of £44,000,000. After feeding the people of Northern Ireland and the troops stationed there the farmers have sent about £10,000,000 worth of food, including 360,000,000 eggs, to swell the larder in Great Britain. The Northern Ireland Ministry of Agriculture has pushed ahead with a scheme of potato processing plant, and it is hoped that nine factories will be completed in time to deal with the 1942 surplus crop, converting it into potato meal for animal feeding and into potato for human consumption.

Depths of the Arctic Ocean

A NOTE on some important Soviet flights in the Arctic is contained in the *Polar Record* of July. In March 1941 Ivan Cherevichni, and four others, flew from Moscow via Archangel, Franz Josef Land and Severnaya Zemlya to their base on Wrangel Island. Thence several flights were made to the north to investigate conditions in the least known area of the Arctic Sea. On the first flight a descent on the pack was made in lat. 81° 2′ N., long. 180° E., and for four days hydrographical observations were made. The ocean depth was 1,447 fm. The next flight was to lat. 78° N., long. 176° 40′ E., where the depth was 1,015 fm., and the third flight was to lat 78° N., long. 170° E., where the depth was 1,878 fm. In the Cherevichni triangle, made by these three stations, the depths appear to be considerably less than the single sounding of Sir Hubert Wilkins in 1927 in lat. 77° 45′ N., long. 175° W., which was 2,830 fm. These discrepancies in depths were not expected in the Arctic Ocean. The physicist in the party found the intermediate layer of warm Atlantic water at all three stations. It will be recalled that this layer, first found by Nansen, was noted by Papanin, of the Soviet drifting polar station, some years ago. It is now fair to assume that it occurs throughout the Arctic basin.

The Couvade

IN the recently published September issue of *Folk-Lore*, Mr. L. F. Newman discusses the curious but widespread and ancient custom named 'couvade', which consists in the illness of the husband during his wife's pregnancy, parturition and puerperium. He classifies the theories as to its origin in three groups according as they are held by (1) the classical school, who follow Apollonius Rhodius, Strabo, Plutarch and the other early writers; (2) the historians, who base their views on authors such as Marco Polo, Bacon, Butler and others; and (3) the anthropologists, who are influenced mainly by scientific data and records from the life of the more primitive peoples. Mr. Newman comes to the conclusion that a number of quite diverse customs have been grouped together under the name of the 'couvade', and that many are not different types of one definite custom, but are survivals of recorded instances of separate customs and are of multiple origin.

Standardizing Electrical Instruments

IN a paper read on December 3 in London before the Institution of Electrical Engineers, Lieut.-Col. K. Edgcumbe dealt with the standardization of industrial electrical instruments from the aspects of performance limits, dimensional limits and certain constructional features facilitating interchangeability and promoting commercial manufacture, and other minor matters such as preferred terms, definitions, symbols, etc. Comparisons are made of the standards issued in Great Britain, the United States and Germany and by the International Electrotechnical Commission, suggestions being put forward for the extension or amendment of the pertinent specifications issued by the British Standards Institution, notably *BS*81, *BS*89 and *BS*90.

Announcements

WE regret to announce the death of Sir Henry Miers, F.R.S., Waynflete professor of mineralogy in the University of Oxford during 1895–1908, principal of the University of London from 1908 until 1915, and vice-chancellor of the University of Manchester and professor of crystallography during 1915–26, on December 10, aged eighty-four.

DR. G. A. R. KON has been appointed as the first occupant of the University of London chair of chemistry tenable at the Royal Cancer Hospital (Free), which was instituted just before the outbreak of war. Dr. Kon has been a member of the staff of the Imperial College of Science and Technology since 1925, the title of reader in organic chemistry being conferred on him in 1935.

IT is announced that the estate of Sir Joseph Larmor, who died on May 19 (see NATURE, June 6, p. 631), amounts to £54,500. He made the following bequests, among others: £3,000 to St. John's College, Cambridge, for annual rewards for undergraduates; £3,000 to the Royal Academical Institution, Belfast, to supplement scholarships; £3,000 to augment emoluments held by junior members in Northern Ireland of Queen's University, Belfast; £2,000 to the University of Cambridge to provide medical and other assistance to junior members of the University; £1,000 each to St. John's College, Cambridge, and Magee University College, Londonderry; and £750 each to Trinity College, Dublin, and the National University of Ireland for the benefit of University College, Galway. Subject to other bequests, the residue is to be used for increasing the educational bequests to St. John's College, Cambridge, the Royal Belfast Academical Institution, and Queen's University, Belfast.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

Discoveries by Accident

PROF. H. R. ROBINSON writes in NATURE of November 21, p. 591: "In November 1895, Röntgen discovered X-rays, largely by accident. Becquerel's discovery of radioactivity in February 1896 was largely a second accident arising out of Röntgen's discovery".

One is tempted to ask, how can it be determined whether a discovery has been made by accident? My colleague in chemistry has provided the following instance of what may legitimately be called an accidental discovery: the fact that mercury acts as a catalyst in the oxidation of naphthalene to phthalic acid by sulphuric acid was revealed by the breaking of a mercury thermometer.

In his "Theory of Light", Preston refers to Arago's discovery in 1811 of the interference of polarized light in the following terms: "Placing by chance a thin plate of mica in the path of a pencil of plane-polarised light and examining it through a doubly refracting prism he observed that both the ordinary and the extraordinary images were richly coloured. . . ." It may have been *by chance* that mica was at hand, but few would call this an accidental discovery because of the associated work and thought of the experimenter.

In the case of Röntgen, no one would gainsay that he was fortunate, lucky perhaps, in having a fluorescent substance on the table, but it may be questioned whether we should call his discovery an accident. According to Glasser, "Röntgen had taken up the work with cathode rays because he had a feeling that there were still many unsolved problems connected with them. . . ." There lay the intention to enter a new field of inquiry, but when a man makes an outstanding discovery in a subject absolutely new to him (as shown by his fifty-eight original papers prior to 1895), it is not surprising to find it attributed to accident, luck or good fortune.

With Becquerel and the discovery of radioactivity the case is rather different, for it was clearly his intention to see whether fluorescent substances emitted anything like X-rays. So the discovery was no accident, though Becquerel had wonderfully good fortune in having at hand a unique collection of uranium compounds handed down by father and grandfather; it was a family discovery.

The subject is a difficult one, for national bias can rarely be eliminated in writing of world events; we become aware in fact of the pitfalls besetting the historian. But I hope that this particular issue may be clarified, for it is a recurrent question in radiological circles: Did Röntgen really discover X-rays, and if so, was it an accident? My usual answers until now have been Yes and No.

SIDNEY RUSS.

Barnato Joel Laboratory,
Middlesex Hospital.

PROF. RUSS has quoted a passage from my *summary* of the Rutherford Memorial Lecture; in the lecture itself I went on to pay a tribute to Röntgen and to Becquerel, emphasizing in particular that there was nothing in their general attack on the problems of X-rays and of radioactivity that could be described as in any way "accidental". My only reason for mentioning at all the element of chance was that in this part of the lecture I was directing attention to the remarkable sequence of largely unrelated events which prepared, at exactly the right time, a field of work for Rutherford and a gateway for his entrance into this field.

The lecture, which occupied about an hour, had to be pruned rather drastically to reduce it to the dimensions appropriate to a printed summary. My unskilful pruning, by divorcing a passage from its context, has clearly left the impression of an opinion of Röntgen's and Becquerel's achievements which I neither held nor wished to convey. I am grateful to Prof. Russ for his letter, and for the opportunity to comment upon it, and so, I hope, to remove all traces of this false impression.

H. R. ROBINSON.

Queen Mary College
(University of London).

Retarding Effect of Ghost Formation on Absorption from Subcutaneously Implanted Tablets of Hexœstrol

IT has recently been reported[1] that when tablets of synthetic œstrogens are implanted subcutaneously into bovines, the outer or 'cortical' layer of the tablets becomes infiltrated with a structure apparently composed of relatively insoluble protein. The formation of this structure, for which the name 'ghost' was proposed, should not be confused with the phenomenon of encapsulation of the tablet within a sac of fibrous or connective tissue, which was considered by Geist *et al.*[2] to cause a considerable decrease in absorption from tablets implanted into humans. Whatever may be the effect of encapsulation, it was considered likely that ghost formation would retard absorption, and experimental evidence that this is so has now been obtained.

In connexion with investigations on lactation, the absorption (that is, loss in weight) from tablets of pure hexœstrol weighing very nearly 1,000 mgm. each, which were implanted subcutaneously into bovines for periods ranging from 10 to 105 days, has been determined. The tablets were disk-shaped, approximately 14·4 mm. in diameter, 7·8 mm. thick in the centre and 5·7 mm. thick at the edge.

Contrary to the findings of Emmens[3], Forbes[4] and others on rats, the absorption curve, though approximately linear up to 10–15 days (when about 12 per cent of the tablet had been absorbed), bends sharply at this point and thereafter pursues an approximately rectilinear course corresponding to a much-reduced absorption rate. The initial absorption rate was about 9 mgm./day, but by 20 days the rate had fallen to about 1·5 mgm./day. In practically all cases the tablets have been observed to be encapsulated on removal. Though the initial absorption rate is probably proportional to the surface area of the tablet, it is clear that in the present experiments the observed drop in absorption rate was not due to a decrease in tablet area as postulated on theoretical grounds by Emmens[3] and Forbes[4] for their rat experiments, since, after 63 days absorption, for example, the surface area had decreased by only about 8 per cent.

The question now arises as to whether the decrease in absorption is due to encapsulation, ghost formation or, perhaps, both. If ghost formation retards absorption, it would follow that tablets which had been implanted and removed from an animal would, if re-implanted into the same or another host, lose less weight in a given time than new tablets. The results given in the accompanying table, which are typical of others, bear out this expectation, thus showing that ghost formation is an important cause of absorption inhibition.

COMPARISON OF ABSORPTION RATES FROM NEW AND USED TABLETS OF PURE HEXŒSTROL.

No. of Tablet	No. and duration of previous implantations	Initial weight (mgm.)	Approx. surface area (mm.²)	Absorption (i.e., loss in weight) in 10 days (mgm.)
C12, cRM	—	1031	584	96·0
C12, cLM	—	1012	590	77·0
C12, cRF	1, (22 days)	862	540	6·5
C12, cLH	1, (22 days)	859	562	7·5
C12, cRH	2, (63 days, and 22 days)	822	539	5·0
C12, cLF	2, (63 days, and 22 days)	808	540	10·5

The tablets were subcutaneously implanted into separate sites in the neck region of a heifer for 10 days.

Moreover, from these and other at present unreported results, it appears that the retardation due to ghost formation is of sufficient magnitude to account for the experimentally observed decrease in absorption rate. This, though it suggests that encapsulation, at least under the conditions of these experiments, has little if any effect on tablet absorption, does not provide absolute proof of this. For it is possible to conceive of a retarding effect due to encapsulation being masked by a bigger effect due to ghost formation. This possibility could be tested if it were possible to prevent ghost formation by some method which had no effect on encapsulation. It may be recalled, however, that Emmens[3] observed encapsulation of tablets implanted into rats, but this did not appear to affect the absorption rate.

It is not yet known whether ghost formation occurs in tablets implanted into small animals but, as pointed out previously[1], it seems likely that the slowing up of absorption due to ghost formation may have to be taken into account in human therapy. These results will be reported in greater detail elsewhere.

Some of the hexœstrol tablets used in this work were provided by Messrs. Boots Pure Drug Co., Ltd., through the kindness of Dr. W. F. Short.

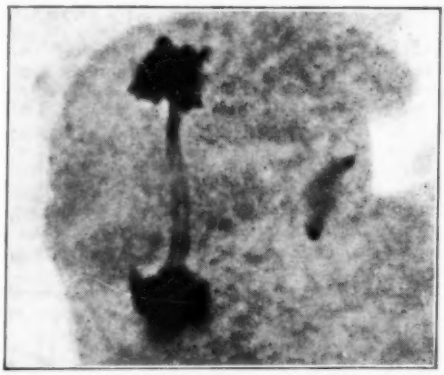
S. J. FOLLEY.

National Institute for Research in Dairying,
University of Reading.
Nov. 24.

[1] Folley, S. J., NATURE, **150**, 403 (1942).
[2] Geist, S. H., Walter, R. I., and Salmon, U. J., *Proc. Soc. Exp. Biol.*, **43**, 712 (1940).
[3] Emmens, C. W., *Endocrin.*, **28**, 633 (1941).
[4] Forbes, T. R., *Endocrin.*, **29**, 70 (1941).

The Telophase Split in Todea

I WAS interested in Dr. Manton's communication with her new and apparently incontrovertible evidence of the telophase split[1]. Her photograph *b* of somatic chromosomes in Todea shows clearly a displaced double strand which has been under a state of tension.



PHOTOGRAPH FROM A PERMANENT ACETO-LACMOID 'SQUASH'[2] OF *Tragopogon pratense* ROOT-TIP SHOWING DOUBLE BRIDGE BETWEEN THE TWO ANAPHASE GROUPS FORMED BY THE DIVISION OF A DICENTRIC CHROMOSOME AND TO ONE SIDE A DOUBLE ACENTRIC CHROMOSOME (× 2000).

The displacement, as Manton points out, is no doubt due in part to squashing, but the stretched double strand is what one always sees with the double chromosome bridges so commonly found in somatic mitosis. For example, in root tips of three-day-old seedlings from two-year-old seeds of *Tragopogon pratense* I have noticed this type of bridge in twenty-two out of seventy-three anaphases. In all cases it was due to the formation of dicentric and acentric chromosomes by interchange in the preceding resting stage. These dicentric chromosomes divide in the three ways described by X-ray experimenters, for example, Koller[2].

The accompanying photograph is of such a dicentric chromosome bridge with its corresponding acentrics from *Tragopogon pratense* for comparison with Manton's photographs. The comparison shows that the so-called telophase split in *Todea* must also be interpreted in this way.

The lower end of the double strand in her Fig. *b* certainly presents the appearance of two centromeres which would be expected from the division of a dicentric chromosome. Further, some of the inclusions in this figure are perhaps in reality the acentrics which accompany dicentric bridges and can be of all the expected sizes. The bridges in such anaphases are, of course, always stretched naturally before fixation and not afterwards.

P. T. THOMAS.

John Innes Horticultural Institution,
Merton, S.W.19.
Nov. 13.

[1] Manton, I., NATURE, **150**, 547 (1942).
[2] Koller, P. C., *Genetica*, **16**, 447 (plates VIII*b* and IX*a*) (1934)
[3] Darlington, C. D., and LaCour, L. F., "The Handling of Chromosomes" (London, 1942).

DR. MANTON'S photograph[1], in which she claims to show an "incontrovertible evidence" of the telophase chromosome split, illustrates what can only be a dicentric chromosome. While the long arms form a bridge of the criss-cross type[2], the short arms lie across each other (*b* in photograph). They are the "two distinct separate threads forced widely asunder", and their thickness is the same as that of the long arms (*c* in photograph). The apparent gap between the centromere and the short arm of one

chromosome as it is seen in *c* is due to excessive pressure in squashing the preparation.

P. C. KOLLER.

Institute of Animal Genetics,
University of Edinburgh.
Nov. 18.

[1] Manton, I., NATURE, **150**, 548 (1942).
[2] Husted, L., *Genetics*, **21**, 537 (1936).

IT is perhaps unnecessary for me to inquire into the reasons for the assumption that the evidence from dicentric bridges in Tragopogon is incompatible with the existence of a telophase split in Todea, for the suggestion that the chromosome figured by me was a dicentric is not borne out by anything in the specimen itself. The accompanying diagram, based on my previously published photograph *b*, shows the region referred to. The two distorted chromosomes are labelled *chr* 1 and *chr* 2, *chr* 1 being that under discussion. In it "two distinct and separate threads have been forced widely asunder", the separation starting at a point near the intersection with *chr* 2 (which is not otherwise of interest) and continuing to the distal extremity of the chromosome. Near this end both threads experience a sudden bend and the only point of uncertainty is whether or not they also cross over each other at this point.

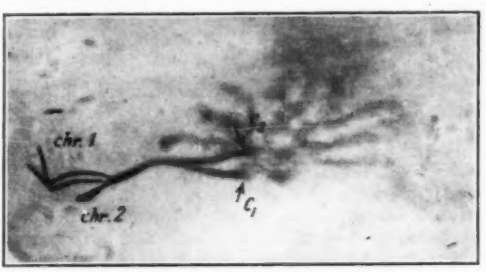


DIAGRAM DRAWN OVER A MATT-SURFACED PRINT OF THE PREVIOUSLY PUBLISHED PHOTOGRAPH *b*. FOR EXPLANATION OF LETTERING SEE TEXT.

The centromeres in both *chr* 1 and *chr* 2, as in all other chromosomes of this plant, are terminal or nearly so and their approximate positions are indicated by arrows and the letters *c* 1 and *c* 2. Owing to the predominance of terminal centromeres the question of short arms, in the form raised by Dr. Koller, does not arise. There are likewise no acentric fragments present, though some cytoplasmic inclusions may perhaps give the illusion of such in the reproduction and be the objects referred to by Dr. Thomas.

The suggestion made by Dr. Koller (if I have understood him aright) that the distal bend is a centromere is discountenanced by comparison with the sister nucleus. The significant part of this was shown in my previous photograph *a*. Had I anticipated that the number and position of centromeres would have been under discussion it would have been easy to show every chromosome in this nucleus by using two or three different focal levels. The level chosen was thought to be sufficient to indicate that the chromosomes were clearly distinguishable and were not disturbed in position. The full haploid complement of chromosomes was present in this nucleus, and therefore if *chr* 1 in the other plate was at any time a bridge it would be necessary to assume it to be a bridge which has broken and been displaced without leaving a trace of its former presence in the sister cell. This appears to me to be highly improbable and has not indeed been claimed.

My opinion that the cell in question was normal is naturally based on a wider knowledge of the material than the one specimen described. I am also acquainted with the appearances caused by dicentrics and fragments in some other organisms, but I have never encountered a case of the kind in the Osmundaceæ. Some positive evidence would therefore be required before one could assume the spontaneous production of chromosomal abnormalities of this kind in a healthy germination of fresh spores from a normal plant.

Further discussion may perhaps be left to the fuller statement which, as indicated in my former note, will be published elsewhere. Additional evidence, which is abundantly to hand, can then more easily be provided.

I. MANTON.

Botany Department,
University of Manchester.
Nov. 25.

Catalysts for Sodium Chlorate in Weed Destruction

BATES[1] has shown that vanadium pentoxide acts as a catalyst for sodium chlorate in weed destruction, and suggests the possibility of enhancing the value of this salt as a herbicide by the employment of a catalyst.

Compounds of manganese, cobalt and nickel act as catalysts in many oxidation processes. These metals frequently occur in plant tissues, and the first two are known to play an important part in their life processes, probably on account of their catalytic action. The effect of compounds of these metals on sodium chlorate, and on a proprietary weed-killer, 'Atlacide', which contains sodium chlorate and calcium chloride, has been investigated. The experiments included laboratory and small field tests. A few tests were also carried out with vanadium pentoxide.

The laboratory tests were made on the trailing stems of the Canadian blackberry, the wild dog-rose, and the wild bramble. The cut ends of the stems were placed in tubes each containing 50 c.c. of the solution under test. In most cases ten specimens were tested with each solution. The tests were as follows:

(1) Canadian blackberry stems in solution containing 10 per cent of sodium chlorate, and also with the addition of 0·03 per cent $MnCl_2.4H_2O$*; 0·03 per cent $MnSO_4.4H_2O$*; 0·02 per cent $KMnO_4$; 0·04 per cent $CoCl_2.6H_2O$*; 0·04 per cent $NiSO_4.6H_2O$*.

(2) Canadian blackberry stems in solution containing 5 per cent of sodium chlorate, and also with the addition of 0·015 per cent $MnCl_2.4H_2O$; 0·015 per cent $MnSO_4.4H_2O$; 0·01 per cent $KMnO_4$; 0·02 per cent $CoCl_2.6H_2O$; 0·02 per cent $NiSO_4.6H_2O$; 0·03 per cent $MnCl_2.4H_2O$; 0·03 per cent $MnSO_4.4H_2O$; 0·02 per cent $KMnO_4$; 0·04 per cent $CoCl_2.6H_2O$; 0·04 per cent $NiSO_4.6H_2O$.

(3) Wild bramble stems in solution containing 2·5 per cent of sodium chlorate, and also with the addition of 0·015, 0·03, and 0·06 per cent $MnCl_2.4H_2O$.

(4) Wild bramble stems in solution containing 2·5 per cent of sodium chlorate, and also with the addition of 0·015 and 0·03 per cent $MnCl_2.4H_2O$; and 0·01 per cent V_2O_5.

(5) Wild bramble stems in solution containing 2·5 per cent of sodium chlorate, and also with the addition of 0·06 per cent $MnCl_2.4H_2O$; 0·08 per cent $NiCl_2.6H_2O$; 0·08 per cent $CoSO_4.6H_2O$.

(6) Wild dog-rose stems in solution containing 2·5 per cent of sodium chlorate, and also with the addition of 0·015 and 0·03 per cent $MnCl_2.4H_2O$; and 0·01 per cent V_2O_5.

(7) Wild bramble stems in 1 part 'Atlacide' concentrate to 3 parts water, and also with the addition of 0·03 per cent $MnCl_2.4H_2O$; and 0·04 per cent $CoCl_2.6H_2O$. The diluted concentrate contained 4·825 per cent $NaClO_3$.

(8) Wild bramble stems in a solution containing 0·06 per cent $MnCl_2.4H_2O$; 0·04 per cent $KMnO_4$; 0·08 per cent $CoSO_4.6H_2O$; 0·08 per cent $CoCl_2.6H_2O$; 0·08 per cent $NiSO_4.6H_2O$; 0·08 per cent $NiCl_2.6H_2O$.

* Approximately equal to 0·02 per cent of the anhydrous salt.

A 10 per cent solution of sodium chlorate is stronger than that usually used on this railway, therefore weaker solutions were also employed. The 2·5 per cent solutions were chosen because it was thought that such dilute solutions would not kill the specimens, or would do so very slowly, and that the effect of the catalysts would be more marked. The action was actually somewhat delayed, but all the specimens were killed in 10–18 days, and the action of the catalysts was not appreciably more marked than with the stronger solutions.

Similar results were obtained with all the catalysts. In each case the lethal effect of the chlorate was increased, as shown by the rate at which the specimens died, and the changes which took place in the leaves and stems. The vanadium pentoxide appeared to be somewhat more potent, and to make a more marked difference to the appearance of the plant; the leaves were more blackened and the stems more stained. Little, if any, difference could be observed between the effects of the other catalysts used. Although some effect was observable with 0·01 per cent of catalyst (calculated as anhydrous salt), there was more with 0·02, and still more with 0·04 per cent.

Different strains of the same species showed a marked difference in their powers of resistance. Thus, in test (2), a few stems were still but little affected when all the others were dead. The resistant specimens were all similar to each other in appearance, but differed considerably from the remaining stems. Similar results were obtained with the dog-rose and the wild bramble.

As was to be expected, the solutions of the catalysts alone had no appreciable effect upon the stems. Bates obtained the same result with vanadium pentoxide, and dilute solutions of the other compounds used are known to have a beneficial effect upon plants.

The field tests were carried out on plots each 4 sq. yd. in area, which were chosen so as to be as alike as possible, but considerable differences existed. In each case the weed-killer used was 'Atlacide'. The tests were as follows:

(1) Plots treated with
1 pint 'Atlacide' (1 part concentrate to 3 parts water); 1 pt. containing 0·015 per cent $MnSO_4.4H_2O$; 1 pt. containing 0·03 per cent $MnSO_4.4H_2O$.

(2) Plots treated with
1 pint 'Atlacide' (1 part concentrate to 5 parts water); 1 pt. containing 0·015 per cent $MnSO_4.4H_2O$; 1 pt. containing 0·03 per cent $MnSO_4.4H_2O$; 1 pt. containing 0·04 per cent $CoCl_2.6H_2O$; 1 pt. containing 0·01 per cent V_2O_5.

(3) Plots treated with
1 pint 'Atlacide' (1 part concentrate to 6 parts water; 1 pt. containing 0·03 per cent $MnCl_2.4H_2O$. Two plots were treated with each solution.

(4) Plots treated with
1 pint 'Atlacide' (1 part concentrate to 8 parts water); 1 pt. containing 0·06 per cent $MnCl_2.4H_2O$. Two plots were treated with each solution.

Although the differences between the different plots made close comparison difficult, by comparing the effects on particular species, for example, bindweed (*Convolvulus arvensis*), reliable comparisons could be made. It was evident, especially where weak solutions of 'Atlacide' were used, that the manganese salts were effective in promoting the lethal action of the herbicide. The effects of the other catalysts were not so definite, but this was probably because insufficient tests were carried out with these compounds.

The laboratory and small field tests both show that the salts of manganese, cobalt and nickel, as well as vanadium pentoxide, have an intensifying effect upon the action of sodium chlorate as a weed-killer. It is proposed to carry out larger tests on the track next season, in order to ascertain if the catalytic effect is sufficiently pronounced to justify the use of these materials in practice.

I desire to thank the Chief Engineer, Southern Railway, Mr. G. Ellson, for permission to publish this paper, and Messrs. the Atlas Preservative Company for supplying the sodium chlorate and 'Atlacide'.

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[1] Bates, NATURE, 148, 753 (1941).

Index of X-Ray Diffraction Data

IN response to the demand expressed at the Conference on X-Ray Analysis in Industry held at Cambridge last spring[1], the Institute of Physics has undertaken to supplement the index of X-ray powder patterns published by the American Society for Testing Materials[2]. The purpose of the index is to identify rapidly any substance that will give an X-ray powder photograph[3]. The work is being carried out in co-operation with the interested American bodies—the American Society for X-Ray and Electron Diffraction and the American Society for Testing Materials.

A considerable number of laboratories in Great Britain have furnished lists of substances for which they are able to supply data, and the 'editing' is being done at the Cavendish Laboratory. The purpose of this letter is to ask all people who have powder photographs of substances that might be suitable for inclusion in the supplement to communicate with Dr. A. J. C. Wilson, Cavendish Laboratory, Cambridge. A list of the substances already offered may be obtained from him on request.

The X-ray index has already proved to be of considerable industrial importance, and an extensive supplement will greatly increase its usefulness. It is hoped, therefore, that there will be a wide response to this appeal.

W. L. BRAGG
(President, Institute of Physics).
Cavendish Laboratory,
Cambridge.

[1] NATURE, 149, 503 (1942); *J. Sci. Instr.*, 19, 32, 80 (1942).
[2] NATURE, 149, 437 (1942).
[3] *Indust. and Eng. Chem.* (Anal. Edit.), 10, 457 (1938).

Errors in Mathematical Tables

I AM now trying to consolidate for publication, partly in a new American National Research Council quarterly, and partly in a book entitled "A Computer's Guide to Mathematical Tables", the work that I have done during the past twenty years on compiling lists of errors in mathematical tables. I should be glad to be told of any known errors in tables, or to have references to published lists. News of unpublished tables would also be appreciated.

L. J. COMRIE.

Scientific Computing Service, Ltd.,
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London, W.C.1.

RESEARCH ITEMS

Starlings and Foot-and-Mouth Disease

THERE is a body of circumstantial evidence to support the view that the starling, *Sturnus vulgaris* (L.), is concerned with the spread of foot-and-mouth disease in cattle, and this has been reviewed by W. S. Bullough (*Proc. Roy. Soc.*, B, **131**; 1942). It is desirable from this, as from more general points of view, that the life-history and habits of this bird should be studied as fully as possible. The reproductive cycles of the British and Continental races of the starling have been investigated in considerable detail by Dr. Bullough (*Phil. Trans. Roy. Soc.*, B, **231**; 1942). The British Isles have their own resident population of starlings, but during autumn and early winter this is augmented by invasions from the Continent. It is possibly significant that Scotland, where foot-and-mouth disease is rare, receives the bulk of its immigrants from Norway, where the disease is almost non-existent. England, on the contrary, where the disease is common, receives its influx from north Central Europe, where the disease is prevalent, and many unexplained outbreaks start in the late autumn after the Continental migration of starlings has set in. The residents and migrants constitute two different races which can be distinguished in both sexes by minor differences, but there is also a difference in their reproductive cycles. The sexual cycle of the British race commences earlier and proceeds more rapidly, with the consequent earlier and quicker development of the breeding characters. British starlings pair in autumn but the Continental ones do not do so until they return to their nesting places in March, and thus there is no interbreeding between the two races (see also NATURE, June 20, p. 683)

Great Barrier Reef Bores

VOL. 5 of the Reports of the Great Barrier Reef Committee (dated April 30, 1942, and issued from Brisbane) has been drawn up by H. C. Richards and Dorothy Hill. They describe the detailed examination of two bores taken at Michaelmas Cay, 23 miles north-north-east of Cairns in 1926 and at the southern end of Heron Island in 1937. General accounts have already been published. The sites were 700 miles apart and the bores were put down to a depth of 600 ft. and 732 ft. respectively. The results of these boring operations are remarkably alike. The occurrence of none but species of corals and molluscs which are found living in the same region to-day indicates that the bores passed through materials deposited entirely during recent times. It is shown that a subsidence of about 100 fathoms almost certainly must have occurred, and this subsidence and the deposition must have been rapid. The most important characters the bores have in common are the great thickness of reef materials in the upper part of each, the occurrence of reef corals through the top 506 ft. in the Heron Island bore and the top 476 ft. of the Michaelmas Cay bore, the occurrence of quartz-foraminiferal sands below the reef rock and the absence of a basement even at 732 ft. The absence of dolomitization, the presence of two aragonite zones and the lack of sufficient cohesion in the material to yield cores comparable with those obtained at Funafuti and the Bahamas show that similar conditions have affected the materials since their deposition.

Vascular System of *Xenopus laevis*

WHILE *Xenopus lævis* is the common form of Anuran amphibian studied in courses of biology in South Africa, only very inadequate and incomplete accounts of its blood vascular system were available until Naomi Millard's account appeared (*Trans. Roy. Soc. South Africa*, **28**; 1941). This is a concise description of the arteries and veins of this species, fully illustrated by diagrams that are all the more clear since the vessels are shown in colour. Xenopus is an interesting species as it is a member of the Aglossa, whereas the previous accounts of the blood vessels of Anurans have been based on members of the Phaneroglossa. Investigations into other anatomical features of the group have suggested that it occupies a primitive position in the Anura. While the main plan of the vessels is undoubtedly Anuran, it is interesting to note that in certain details, particularly the veins of the hind limb and the abdominal veins, relationships are present that suggest conditions otherwise encountered in the Urodela, for example, *Salamandra salamandra*. The author concludes that these and other similarities do not imply close affinity but are rather due to both animals being neotenic. Some of the other peculiarities and abnormalities encountered will probably find explanations when the developmental history of the system is worked out. The abnormalities, a number of which are described, appear to occur in a greater percentage of the specimens than they do in the Ranidæ.

Effect of Colchicine

J. G. HAWKES (*J. Genetics*, **44**, 11–22; 1942) has shown that colchicine only exerts its effect on those mitotic stages where a spindle mechanism is present. The centromeres of chromosomes treated with colchicine do not divide until the resting stage which follows the treatment, while the succeeding metaphase is followed by the formation of a lobed restitution nucleus. The orientation of the spindle in the root tip is distorted by colchicine, and this gives rise to the production of a swollen root tip. Absence of polarity of colchicine-treated cell divisions is a cause of the noticeable swellings of the roots. The duration of a cell-division cycle after colchicine is 33 hours in *Allium cepa*, which is about twice as long as a normal cell division in comparable circumstances.

Intergeneric Hybrids of Saccharum

SEVERAL hybrids between *S. officinarum* and *Narenga porphyrocoma*, made by C. A. Barber in 1913, have been analysed by E. K. Janaki-Ammal (*J. Genetics*, **44**, 23–32; 1942). The parents have $2n = 80$ and $2n = 30$ chromosomes respectively. The hybrids have $2n = 55$ chromosomes. Chromosomes in the hybrid show autosyndesis and associate as quadrivalents, trivalents and bivalents. This may be the cause of male and female sterility in the hybrids, which would appear to be similar to the hybrid form 'Hitam Rokham' which is found wild.

Origin of Tetraploid Shoots in *Brassica oleracea*

H. W. HOWARD (*J. Genetics*, **44**, 1–9; 1942) has analysed the origin of tetraploid shoots in *Brassica oleracea* after decapitation. New meristems arise from the cells of the callus, which were produced by large vacuolated cells. No increase in the proportion of the tetraploid shoots has been induced by treatment with hetero-auxin. The author considers it

probable that tetraploid meristems arise from cells of the stem which contained diplo-chromosomes before injury, and that decapitation as such did not induce chromosome doubling.

Structure of Lead Chromate

THE work of Brill (*Z. Krist.*, **77**, 506; 1931) showed that lead chromate (crocoite) is monoclinic, that there are four $PbCrO_4$ molecules per unit cell, and that the probable space group is $C_{2h}^5 - P2_1/n$. S. B. Brody (*J. Chem. Phys.*, **10**, 650; 1942) has confirmed these data, which indicate that lead chromate cannot be classified with any other oxy-acid substance so far investigated. Simple rotation and Weissenberg equi-inclination X-ray photographs were taken about the *b* and *c* axes, using unfiltered copper radiation. The intensities were carefully corrected for polarization and absorption. Parameters for the lead and chromium atoms have been obtained but the electron density maps were insufficiently accurate to locate the oxygen parameters. There is evidence, however, that the structure is fairly close packed and that the co-ordination number of oxygen atoms about lead is probably 8 or more. It is concluded that the mechanism of photo-conductivity and photo-blackening in lead chromate must take place by migration of ions by means of holes due to lattice defects.

Counting Free Alkyl Radicals

AN interesting method, whereby free alkyl radicals produced by photolysis can be counted, has just been published by M. H. Feldman, J. E. Ricci, and M. Burton (*J. Chem. Phys.*, **10**, 618; 1942). Inside a reaction tube, long, heavy mirrors of lead are deposited and the lead is then carried from this tube by free alkyl radicals (Paneth method). The lead alkyl is brought in contact with a mixture of bromine and carbon tetrachloride whereby it is converted to lead bromide. Volatile substances are carefully distilled off and the lead determined by the dithizone method. Thus, assuming that all the alkyl radicals are 'mirror-active' and that only PbR_4 is formed, the number of radicals can be estimated. The half-life of the methyl radicals produced in the photolysis of acetone by 2537 A. mercury radiation at 45° is $1{\cdot}1 \times 10^{-2}$ sec. and at 100°, 8×10^{-3} sec. This difference appears to be due to the instability of free acetyl radicals produced according to $(CH_3)_2CO + h\nu \rightarrow CH_3 + CH_3CO$ and decomposing to CH_3 and CO. The concentration of free methyl radicals flowing past the irradiated portion of the tube has been computed to be approximately $1{\cdot}7 \times 10^{-12}$ mols per c.c. when $2{\cdot}7 \times 10^5$ c.c. of gas passes this point in 5 minutes. The number of mols of methyl picked up during that time at that point is $4{\cdot}6 \times 10^{-7}$.

Differential Electronic Stabilizer for Alternating Voltages

A PAPER read recently in London by Arvon Glynne before the Institution of Electrical Engineers describes the construction of a differential electronic stabilizer, which has been designed to provide a supply unaffected by fluctuations in the mains voltage, for testing A.C. instruments. A simple lamp stabilizer is also described. A bridge consisting of two lamps and two resistors is an essential part of the apparatus, for which the mathematical theory is developed, and experimental evidence of the accuracy of the theory is given. Applications of the differential stabilizer to the testing of D.C. and A.C. indicating instruments and of light- and heavy-current integrating meters are described, and an account is given of the use of stabilized voltages for A.C. potentiometry. Both stabilizers have been in constant use for some time for standardizing instruments, and have proved to be very reliable and to require very little attention. When checking integrating meters, one observer is now able to perform all duties, and it is believed that the precision of the measurements has been materially increased. A single observer is now able to check any number of indicating instruments, as he is assured that the readings will not alter as he passes from one to the other. With the unsteady supplies previously available, the A.C. potentiometer has been regarded as a troublesome and unreliable instrument unsuitable for serious work, but it has now taken its proper place as a precision instrument. The stabilizer has been developed principally for use with single-phase supplies, but it can also be used with equal success for checking polyphase instruments.

Calculation of Precession

STAR catalogues are now reduced to standard equinoxes at intervals of twenty-five years, and tables specially relating to the two epochs are compiled for the purpose, or more rigorous formulæ of spherical trigonometry are used. In the case of stars of high declination, where a formal accuracy of at least 0·01^s in R.A. is desirable, it is often necessary to utilize the rigorous method. In these circumstances, where the spherical triangle involved is small, Legendre's theorem can be applied, and the formulæ used produce errors which *decrease* as the pole is approached. This theorem states that in cases where the sides of a spherical triangle are small compared with the radius of the sphere, the spherical triangle may be replaced by a plane triangle, the sides of which are of the same length as the arcs of the spherical triangle, but each angle is less by one third of the spherical excess than the corresponding angle of the spherical triangle. This principle has been applied in geodetic operations but has not been used in astronomical computations. D. H. Sadler, superintendent of H.M. Nautical Almanac Office, has now shown (*Mon. Not. Roy. Astro. Soc.*, **102**, 4) how the principle leads to some simple formulæ which can be used for the reduction of star positions from one equinox to another. They possess the great advantage that accuracy of computation increases as the pole is approached, and they are easier to use than the ordinary rigorous formulæ employed in spherical trigonometry.

Galactic Clusters in Relation to the Galaxy

As a result of his investigations, George Alter concludes that the observed distribution of cluster numbers is due to a true eccentric displacement of the sun in a real cluster system which is not merely a selection from a general random distribution (*Mon. Not. Roy. Astro. Soc.*, **102**, 4). The centre of this cluster system is about 250° longitude. The asymmetrical distribution of absorption conditions in longitude as well as the presence of the so-called 'galactic windows' indicate that most of the absorption is due to irregular clouds which accumulate towards the galactic centre in the direction of 350° galactic longitude. In this direction the absorption coefficient increases by about 5 per cent per kilo parsec, but in any other direction the change is smaller.

QUALIFICATIONS OF THE SOCIAL PSYCHOLOGIST

PROF. T. H. PEAR criticizes severely but kindly in an interesting article those who profess social psychology*. The social psychologist too frequently fails to qualify for his job by obeying the old injunction of 'know thyself'. Also in his attitude towards his subject he is too remote from the hurly-burly of everyday life, in fact, too 'pure'. Some sciences can be pursued advantageously with little contact with, or relation to, humanity, but such is not the case with social psychology. On the other hand, he must resist the temptation to slip into the attitude of the novelist, artist or even comfort lover.

The direction of any science is determined, to an extent not usually admitted, by the inborn and acquired mental peculiarities of its leaders and by their social and economic status. Most social psychology has been produced in Great Britain by members of the middle class, and they have too often been unaware of this limiting factor. Examples of this attitude of mind have sometimes been thrown into relief during the War. For example, in discussing fuel saving, some people who had never known what it was not to be able to have a bath when they wanted one, suggested a nation-wide campaign urging people not to have more than two hot baths a week, in blind ignorance that there were numerous bathless houses.

Leaders of thought brought up in a particular environment are apt to think that certain subjects are important or trivial, popular or dull, in general, though a different culture group might hold quite different views. A social psychologist must not be 'tuned in' merely to those expressions of social life which the conventions of his social milieu regard as important. For example, the subjects of speaking and conversing are generally neglected. That such problems as standard English, the prejudices for and against certain kinds of dialect, are social, is unrealized. To delve into them would break a taboo all the stronger because in most people it is unconscious.

Therefore, before entering upon any research in social psychology the investigator should know his own social background, and the ways in which it may have given him special prejudices.

Industrial research is not a bad training in this respect. If the investigator is in one factory for a considerable time, he will have ample opportunities for finding out, incidentally to his main work, what people do think, or say they think, about the topics that are of current interest. A point of importance in this connexion is that much of what is said has to be translated or interpreted. Some students of public opinion are apt to overlook that, in Great Britain at least, to express an opinion is not synonymous with being prepared to act upon it. Much gossip and expression of opinion is of the nature of a probe to see what other people are like or think.

The somewhat humdrum task of finding out how we ourselves behave in particular circumstances, and how others do, is vitally necessary for the development of social psychology.

Prof. Pear thinks that too many men of science believe that those who are not trying to establish general laws are wasting time, even when there may be a serious shortage of facts in the particular subject. Even books about the human mind are often of this kind and suffer from a paucity of examples. Social psychologists have no right to neglect the observation, recording and classification of concrete data. Their scientific training would be shown in the way they used their data. In short, they must not spend their days in the study, but frequent the market-place, and know their own blind spots, mental and social.

* The Social Status of the Psychologist and its Effect upon his Work. By Prof. T. H. Pear, *Sociol. Rev.*, **34** (Jan.–April, 1942).

SCATTERING OF LIGHT

THE interest which for many years now has been displayed by Indian workers on phenomena of light scattering has produced a large volume of valuable observations and many important and far-reaching results. While, since 1928, investigations have been predominantly concerned with different aspects of the Raman effect, other branches of light scattering have received attention. Many individual papers have appeared, but periodically a symposium of papers is published. These are most helpful in weaving together the various topics. The one under review* on "Fluorescence, Light Scattering, and the Raman Effect" worthily follows the traditions created by previous symposia and maintains the same high standard.

The symposium opens with two papers by P. G. N. Nayar on the absorption and scattering of light in diamonds. His earlier work had shown that both in fluorescence and absorption the entire spectrum is closely related to the 4152 A. band. Examination of these spectra at liquid-air temperature has now revealed the existence of a considerable number of discrete lines in the spectra with frequency shifts varying from 178 to 1330 cm.$^{-1}$ and, further, has shown the perfect mirror-image symmetry in the shifts and in the intensities of these lines with respect to the 4152 A. line. These data enable the author to derive the full lattice spectrum of the diamond, and the discrete character of this spectrum postulates that atomic oscillations in a simple crystalline solid are of a different nature from those of an elastic solid. Further, the derived spectrum contains practically all the known Raman and infra-red frequencies and indicates that the spectrum is richer in frequencies than had been supposed. The investigation of the ultra-violet absorption in the temperature range 85°–300° has demonstrated that the 4152 A. line is a doublet, that between 3015 and 3447 A. there are some twenty-five distinct lines, that below 3000 A. there is an unresolved group, and that the influence of temperature on all lines is of the same order of magnitude. Investigation of the changes in frequency, intensity, and polarization of the 1332 cm.$^{-1}$ Raman line with crystal setting and angle of scattering show that the frequency and intensity are unaltered. The depolarization factor is 1, not 2 as obtained theoretically. One other interesting point emerges in the constancy of the intensity and frequency of the 1332 cm.$^{-1}$ line in diamonds exhibiting widely different fluorescence and absorption properties.

B. S. Satyanarayana discusses the fluorescence of uranyl compounds. In the solid state the fluorescence spectrum consists of eight discrete bands having a wave number separation of approximately 860 cm.$^{-1}$. The Raman spectrum of the uranyl UO_2^{++} ion con-

* *Proc. Indian Acad. Sci.*, *A*, **15**, 293–416 (1942).

sists of three displacements of 211, 865, and 909 cm.$^{-1}$. It is concluded from an analysis of the Raman data that the ion has a symmetrical, bent structure.

More than workers in other countries, Indian physicists have directed attention to the interesting changes in the Raman spectra of crystals on account of different crystal settings, and of the influences of the directions of the incident and scattered radiation. T. M. K. Nedungadi has extended our knowledge on this topic by an exhaustive investigation of the Raman effect in a single crystal of naphthalene. The spectrograms obtained for various settings of the crystal with respect to the directions of the incident radiation and the line of observation and with various directions of vibration in the incident and scattered radiation are reproduced. Experimental data confirm predictions of group theory that six low-frequency lattice oscillations exist in three pairs of close doublets: 38, 47; 72, 78; and 107, 121 cm.$^{-1}$. The higher member of each pair arises from oscillations symmetrical to the elements of symmetry of the monoclinic lattice, whereas the lower members of the pairs are due to antisymmetrical oscillations. On frequency and intensity considerations these pairs of lines correspond roughly with rotational oscillations of the molecule. Theoretically, naphthalene should possess twenty-four normal modes of vibration and these have been identified. Polarization data have shown that some of the internal oscillations, particularly 512 and 1576 cm.$^{-1}$, split up into symmetric and antisymmetric components; this is interpreted as showing that polarization data are controlled, as are lattice oscillations, by symmetry properties.

Work continues on determination of the Raman frequencies and their intensities and polarization characteristics and on deducing the characteristic Raman frequencies of chemical groups and linkages. Contributions in this direction are made by Dr. C. S. Venkateswaran and N. S. Pandya with data on aniline, diethyl sulphide, and two binary liquid mixtures. The binary mixtures investigated—*cyclo*-hexane-aniline and *iso*-propyl alcohol-water—possess a critical solution temperature and, from changes in the position and intensity of five lines of the second mixture, it is concluded that at the critical solution temperature only monomeric molecules of *iso*-propyl alcohol exist. This is an interesting new departure in Raman spectral investigations, and shows that the directions in which the methods of Raman spectra can be applied to the investigation of chemical problems are, indeed, manifold.

The remaining papers of the symposium deal with the scattering of light in gases, in mobile and viscous liquids, in glasses, in alcohols, and in emulsions. The light scattered in a transverse horizontal direction by dilute emulsions is usually elliptically polarized, whereas the scattered light from concentrated emulsions is highly depolarized. In a large variety of liquids hypersonic velocities have been determined and compared with acoustic velocities. K. S. Bai has examined existing theories of light transversely scattered by liquids and has derived thermodynamically a general expression for the intensity.

The contents of the papers constituting the symposium will prove extremely valuable not only to all directly concerned with this field of scientific inquiry, but also to a wide circle of chemists and physicists interested in the applications of these methods to a variety of problems.

W. ROGIE ANGUS.

THE LOCUST PLAGUE

By DR. A. D. IMMS, F.R.S.

ONE of the oldest of all agricultural problems is that of locust invasions. References to such plagues are frequent in the texts of the ancient Egyptians, Greeks, Chinese and Hebrews—the oldest dates back to more than 2000 years B.C. These depredations are not confined to a few countries, although outbreaks regularly appear in individual lands only to devastate many distant territories. It cannot be emphasized too strongly that much of the failure to control these plagues has been due to isolationist policy adopted by those countries directly affected.

This feature was stressed by Dr. B. P. Uvarov in a lecture delivered by him before the Dominions and Colonies Section of the Royal Society of Arts on December 15. As Dr. Uvarov pointed out, history provides examples of great efforts to control locusts in Algeria, Argentina, South Africa, etc., with only temporary results simply because the problem is insoluble within the confines of a single country. He mentions the example of swarms of the desert locust, bred in India, that usually migrate to Persia and Arabia and their descendants proceed to spread over Egypt, Palestine and East Africa.

It is now generally agreed among investigators, and the more enlightened administrators of locust control methods, that international co-operation affords the only sure method leading to the ultimate and permanent solution of the problem. Sporadic attempts to approach some measure of international co-operation have not been lacking, but no practical effect resulted owing to failure to take scientifically planned concerted measures. In 1928 a severe outbreak of the desert locust was the moving factor in the British Government deciding to take steps that might lead to a solution of the causes of the periodical swarming of locusts and the resulting depredations. A Locust Sub-Committee of the Committee of Civil Research (later transformed into the Committee on Locust Control of the Economic Advisory Council) was formed. The actual work was undertaken by a special research unit under the supervision of Sir Guy Marshall and under the technical supervision o Dr. B. P. Uvarov. The history of this important venture and the results so far obtained were dealt with in the lecture referred to. In a few words it may be said that this purely British organization soon attracted attention in other lands.

The first International Locust Conference at Rome in 1930 requested the British organization to act as the centre of anti-locust research. Concerted investigations by parties of British, Belgian, South African, Egyptian and Indian experts have gradually led towards a fundamental understanding of the whole problem. After eight years of intensive work it has become possible to formulate a policy that aims directly at a radical solution of the difficulties. The investigations referred to have yielded abundant data showing that the periodicity of locust outbreaks is closely connected with transformation of these insects from the harmless solitary phase into the dangerous migratory, gregarious one. The transformation can happen only in certain localized areas with special conditions of climate and vegetation which differ for different locust species. It is in these outbreak areas that the first swarms are formed. The important discovery that great locust invasions

have such restricted beginnings has vital implications. It means that once these original outbreak areas are known they can be kept under proper observation, and any tendency towards the formation of incipient swarms can be dealt with before such swarms can spread and cause widespread devastation. It cannot be too strongly emphasized that Dr. Uvarov himself is the guiding agent in all this international locust policy, as it was his original phase theory that provided the scientific basis for the new outlook on the problem as a whole.

STUMP PRODUCTION IN MADRAS TEAK NURSERIES

WHAT has come to be known as stump planting' that is, the use of root and shoot cuttings, has been long known in India, some of the earliest experiments made in this direction having been successfully attempted with the sissu (*Dalburgia Sissoo*) in the irrigated plantations in the Punjab. In *Indian Forest Records*, **4**, No. 5, 1941 (Manager, Government of India Press, New Delhi, 1942), A. L. Griffith says that the planting of stumps in Madras displaced direct dibbling of seed and transplanting, as had been done up to that time. In the early days of stump planting in the presidency a number of experimental investigations were undertaken and continued over some years. Mr. Griffiths summarizes the results in the following tentative conclusions for work in a general West coasts type of climate :

(*a*) The best density of sowing to use is that of 1/8 lb. to 1/5 lb. of teak seed per square foot of nursery bed, and to do no pricking out of seedlings at all. This is most economical in quantity of seed and results in a high proportion of the stumps produced being of the best size for planting at 1 year old (that is, 0·4–0·8 in. diameter). (*b*) Thus, one standard 40 ft. × 4 ft. nursery bed sown evenly with 20–30 lb. of seed *on the average* produces enough well-shaped stumps of usable size to plant 1 acre of plantation at a 6 ft. × 6 ft. espacement and also leaves a reserve for casualty replacement. (*c*) Poor seed years have to be guarded against, and at the end of the first year either the undersized plants can be left in the bed and these together with germination from seed still dormant will produce a full bed of good-sized plants at the end of the second year, or the undersized plants can be pulled out with the bigger ones, stumped, and put back into a reformed nursery bed. These plants at the end of the second year will give a high proportion of well-shaped, good-sized stumps which are in no way inferior to fresh, good-sized one year old stumps. (*d*) Although teak stumps store well, and travel well, a large central permanent nursery for the production of stumps for an extensive area does not appear advisable, as in a few years stump production falls off seriously in most localities as a result of continued cropping, and this falling off was not prevented by manuring with artificial manure, or by fallowing, or by growing a green manure crop of sunn hemp in alternate years. In addition, pest attack by cockchafer and white ants may be serious in a permanent nursery and occurred in two out of the five years of experiment.

The conclusion arrived at between the permanent and the temporary or flying nursery is most important and of high interest,

ANALYSIS OF ELECTRICAL CONTACT EFFECTS

IN a paper entitled "Contact Non-Linearity, with reference to the Metal Rectifier and the Carborundum Ceramic Non-Linear Resistor" (*J. Inst. Elec. Eng.*, **89**, Pt. 1, No. 23 ; November 1942), A. Fairweather gives an account of an experimental analysis designed to correlate and formulate the behaviour of various structures possessing contact non-linearity. New data are presented concerning representative contact combinations such as various carborundum combinations, the carborundum ceramic non-linear resistor, the cuprous oxide and selenium rectifiers, and the diamond. Measurements have been made of the variation of the characteristic parameters, resistance and capacitance, with each of the four significant variables—current, mechanical pressure, thickness of semi-conductor and temperature. The ranges of current and voltage far exceed those which have been employed hitherto. Examination of the results obtained leads to various conclusions regarding the mechanism of contact non-linearity, but not, however, in conflict with the familiar 'barrier layer' hypothesis, which is fully supported by the work described.

It would seem that three clearly defined regions of operation hold for conduction in both directions through a contact possessing contact non-linearity. For 'small' voltages and for 'large' voltages conduction is ohmic : for 'medium' voltages, which are largely those of interest, conduction is non-linear, and obeys a simple empirical equation of the form $V = Ki^n$, where n depends mainly on the materials, and K on the dimensions, of the contact combination. This type of law might hold for all contact combinations, and its importance is such as to warrant the development of a theory of circuits containing a non-linear resistor having such a characteristic. Since the contact is never likely to be perfect, the current will be concentrated at a number of spots which may be regarded as imperfections in the boundary. The 'small' voltage ohmic resistance is thus identified as a constant resistance arising from spreading current paths associated with these spots. The 'large' voltage ohmic resistance is due to the resistance of the bulk electrodes, which becomes all-important when the boundary resistance has become sufficiently small. The 'medium' voltage non-linear resistance results from conduction across the nominally insulating portion of the boundary.

The simple gap picture of the boundary seems to have been discarded prematurely. In the case of the cuprous oxide rectifier, where it is generally supposed that the boundary layer consists of highly insulating, oxygen-free cuprous oxide, such gaps might result from intercrystalline or crystalline cracking at the boundary, arising during cooling from the high temperature of preparation, and this picture is in closer agreement with the results of experiment. The importance of two much neglected aspects of contact non-linearity are stressed : first, the capacitance parameter, as a possible source of information regarding the insulating portions of the boundary layer, and secondly, the necessity for using structures which are previously unstressed mechanically, for detailed study.

Carborundum, cuprous oxide, and selenium are all shown to be ohmic conductors, in opposition to the views of some workers. The tests on contacts between

diamonds—a very high-resistance crystalline material—have suggested the probable existence of an apparently anomalous effect, in that the contact resistance in the non-linear region increases with increasing voltage. This is thought to be related to the familiar resistance maximum occurring in the reverse direction with the metal rectifier.

FORTHCOMING EVENTS

Sunday, December 20

WORKERS' EDUCATIONAL ASSOCIATION (GLASGOW BRANCH), ASSOCIATION OF SCIENTIFIC WORKERS (SCOTTISH AREA), WORKERS' EDUCATIONAL TRADE UNION COMMITTEE (SCOTTISH AREA), and the EDUCATIONAL INSTITUTE OF SCOTLAND (GLASGOW ASSOCIATION) (in the Glasgow and West Scotland Commercial College, Pitt Street, Glasgow, C.2).—Joint Conference on "Science and the Struggle for Freedom". At 10.30 a.m.—Prof. H. Levy, Prof. V. Gordon Childe and Dr. C. Ockrent: "Science in the Struggle against Fascism"; at 2.30 p.m.—Dr. Martin Ruheman, Dr. F. Haldane and Mr. C. R. Wason: "Science in the Soviet Union".

Monday, December 21

ROYAL GEOGRAPHICAL SOCIETY (at Kensington Gore, London, S.W.7), at 5 p.m.—Mount Everest Films from the Expeditions of 1922 and 1933 and the Flight of 1933.

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

TEACHER (MAN OR WOMAN) OF MATHEMATICS in the Technical Day School—The Clerk to the Governors, South-West Essex Technical College and School of Art, Forest Road, Walthamstow, London, E.17 (December 23).

ASSISTANT MISTRESS TO TEACH BIOLOGY AND SOME SUBSIDIARY CHEMISTRY AND PHYSICS—The Clerk to the Governors, South-East Essex Technical College, Longbridge Road, Dagenham (December 28).

ASSISTANT LECTURER AND DEMONSTRATOR IN BOTANY—The Secretary, West of Scotland Agricultural College, 6 Blythswood Square, Glasgow (December 28).

WOMAN PSYCHOLOGIST for service at the Child Guidance Clinic—The Chief Education Officer, Education Office, Council House, Margaret Street, Birmingham 3 (January 9).

REGIUS PROFESSOR OF GEOLOGY at Edinburgh University—The Private Secretary, Scottish Office, Fielden House, 10 Great College Street, London, S.W.1 (January 11).

GRADUATE FULLY QUALIFIED TO TEACH PHYSICS—The Headmaster, Royal Liberty School, Romford.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

British Rubber Producers' Research Association. Publication No. 27: Rubber, Polyisoprenes and Allied Compounds. Part 2: The Molecule-Linking Capacity of Free Radicals and its Bearing on the Mechanism of Vulcanisation and Photo-Gelling Reactions. By E. H. Farmer and S. E. Michael. Pp. 8. Publication No. 28: The Interaction between Rubber and Liquids. 3: The Swelling of Vulcanised Rubber in Various Liquids. By G. Gee. Pp. 6. Publication No. 29: The Crystal Structure of β-Isoprene Sulphone. By E. G. Cox and G. A. Jeffrey. Pp. 8. (London: British Rubber Producers' Research Association.) [1111

War-Time Information for Pharmacists. Compiled by the *Pharmaceutical Journal*. Pp. 54. (London: Pharmaceutical Press.) 1*s*. [1211

Scientific Proceedings of the Royal Dublin Society. Vol. 23 (N.S.), No. 4: The Identity of the Top-Necrosis Virus in up-to-date Potato. By Phyllis E. M. Clinch. Pp. 18–34+1 plate. 2*s*. Vol. 23 (N.S.), No. 5: Formation of Gynospore, Female Gametophyte, and Archegonia in *Sequoia*. By W. J. Looby and J. Doyle. Pp. 35–54. 2*s*. 6*d*. (Dublin: Hodges, Figgis and Co., Ltd.: London: Williams and Norgate, Ltd.) [1311

Ministry of Agriculture and Fisheries. Bulletin No. 101: Hedge and Tree-Stump Clearing. By Dr. T. Swarbrick. Fourth edition. Pp. ii+13+4 plates. (London: H.M. Stationery Office.) 6*d*. net. [1811

Royal Meteorological Society. Bibliography of Meteorological Literature. Prepared by the Royal Meteorological Society with the collaboration of the Meteorological Office. Vol. 5, No. 3 (January–June 1942). Pp. 29–46. (London: Royal Meteorological Society.) 2*s*. 6*d*. [2511

Institute of Chemistry of Great Britain and Ireland. Lecture on Rust-, Acid- and Heat-Resisting Steels. By Dr. W. H. Hatfield. Pp. 48. (London: Institute of Chemistry.) [2511

Proceedings of the Royal Society of Edinburgh. Section A (Mathematical and Physical Sciences). Vol. 61, Part 3, No. 17: Axiomatic Treatment of Kinematical Relativity. By Dr. G. C. McVittie. Pp. 210–222. 1*s*. Vol. 61, Part 3, No. 18: Heat Flow in the Region bounded Internally by a Circular Cylinder. By J. C. Jaeger. Pp. 223–228. 6*d*. Vol. 61, Part 3, No. 19: A Short Table of $I(0\,1\,;x)$. By J. C. Jaeger and Martha Clarke. Pp. 229–230. 6*d*. (Edinburgh and London: Oliver and Boyd.) [2511

Freshwater Biological Association of the British Empire. Scientific Publication No. 7: Keys to the British Species of Ephemeroptera with Keys to the Genera of the Nymphs. By D. E. Kimmins. Pp. 64. (Ambleside: Freshwater Biological Association.) 2*s*. 6*d*. [2511

Social Insurance and Allied Services. Report by Sir William Beveridge. (Cmd. 6404.) Pp. 299. (London: H.M. Stationery Office.) 2*s*. net. [212

City and Guilds of London Institute. Report of the Council to the Members of the Institute for the Year 1941. Pp. xiv. (London: Gresham College.) [212

Haemoglobinometers, Haldane Type. (British Standard 1079: 1942). Pp. 12. (London: British Standards Institute.) 2*s*. net. [212

Other Countries

Smithsonian Miscellaneous Collections. Vol. 103, No. 3: A Revision of the Indo-Chinese Forms of the Avian Genus Prinia. By H. G. Deignan. (Publication 3689.) Pp. ii+12. (Washington, D.C.: Smithsonian Institution.) [1311

American Museum of Natural History. Vol. 80, Art. 4: The Osteology and Relationships of the Elephant Shrews (Macroscelididae). By Francis Gaynor Evans. Pp. 85–125. (New York: American Museum of Natural History.) [1611

Brooklyn Botanic Garden Record. Vol. 31, No. 4: Prospectus 1942–1943, including Victory Garden Courses. Pp. viii+211–249. (Brooklyn, N.Y.: Brooklyn Institute of Arts and Sciences.) [1611

Trinidad and Tobago: Forests Department. Administration Report of the Conservator of Forests for the Year 1941. Pp. 4. (Trinidad and Tobago: Government Printer.) 4 cents. [1611

Transactions of the San Diego Society of Natural History. Vol. 9, No. 34: Foraminifera from the Type Area of the Kreyenhagen Shale of California. By J. A. Cushman and S. S. Siegfus. Pp. 385–426+plates 15–19. Vol. 9, No. 35: Two New Wrens and a New Jay from Lower California, Mexico. By Laurence M. Huey. Pp. 427–434. Vol. 9, No. 36: A New Race of the Rusty Sparrow from North Central Sonora, Mexico. By A. J. van Rossem. Pp. 435–436. Vol. 9, No. 37: A New Flea of the Genus Ceratophyllus. By G. F. Augustson. Pp. 437–438. Vol. 9, No. 38: A Pleistocene Tortoise from the McKittrick Asphalt. By Loye Miller. Pp. 439–442. (San Diego, Calif.: San Diego Society of Natural History.) [1811

Records of the Geological Survey of India. Vol. 77, Professional Paper No. 2: A Geological Investigation of Tunnel Alignments for the Jumna Hydro-electric Scheme. By J. B. Auden. Pp. 29+10 plates. (Calcutta: Geological Survey of India.) 2.8 rupees; 4*s*. [2411

U.S. Department of Agriculture. Circular No. 649: Studies on the Prevalence of the European Corn Borer in the East North Central States. By A. M. Vance. Pp. 24. (Washington, D.C.: Government Printing Office.) [2411

Instituto Nacional de Tecnologia. Considerações sobre as propriedades anti-detonantes do alcool e seu emprego como combustivel. Pelos Fernando Affonso Baster Pilar e Armando Silva de Araujo. Pp. 42. (Rio de Janeiro: Instituto Nacional de Tecnologia). [2411

Imperial Council of Agricultural Research. Miscellaneous Bulletin No. 16: Indian Grazing Conditions and the Mineral Contents of some Indian Fodders. Revised and enlarged by Dr. P. E. Lander. Pp. iii+113. 5.6 rupees; 8*s*. 6*d*. Miscellaneous Bulletin No. 49: Investigations on the Cold Storage of Nagpur Oranges. By Dr. D. V. Karmarkar and B. M. Joshi. Pp. iii+17. 12 annas; 1*s*. Miscellaneous Bulletin No. 50: Report on Cardamom Cultivation in South India. By W. Wilson Mayne. Pp. ii+67. 1.10 rupees; 2*s*. 6*d*. (Delhi: Manager of Publications.) [2511

Commonwealth of Australia: Council for Scientific and Industrial Research. Bulletin No. 148: Studies in Fertility in Sheep. 2: Seminal Changes affecting Fertility in Rams. By R. M. C. Gunn, R. N. Sanders and W. Granger. Pp. 140+15 plates. (Melbourne: Government Printer.) [2511

Transactions of the American Philosophical Society. New Series, Vol. 32, Part 1: Contributions to the Archæology of the Illinois River Valley. By Frank C. Baker, James B. Griffin, Richard G. Morgan, Georg K. Neumann, Jay L. B. Taylor. Edited by James B. Griffin and Richard G. Morgan. Pp. iv+208 (68 plates). New Series, Vol. 32, Part 2: Egyptian Planetary Texts, by O. Neugebauer: On Some Astronomical Papyri and related Problems of Ancient Geography, by O. Neugebauer. Pp. ii+209–264+27 plates. (Philadelphia: American Philosophical Society.) [2511

Annual Report of the Indian Central Jute Committee for the Year 1941–42. Pp. iv+218. (Calcutta: Indian Central Jute Committee.) [2511

Indian Forest Leaflet No. 23 (Chemistry): Tamarind Seed, a Valuable Source of Commercial Pectin. By T. P. Ghose and S. Krishna. Pp. ii+7. (Dehra Dun: Forest Research Institute.) 4 annas; 6*d*. [2711

Gold Coast Colony. Report on the Forestry Department for the Year 1941–42. Pp. 4. (Accra: Government Printer.) 1*s*. [212

Catalogues

How Sunvic Hotwire Vacuum Switches Simplify Control in Research and Industry. (Publication Vu.10a.) Pp. 8. (London: Sunvic Controls, Ltd.)

The History of Science Library of Prof. A. Wolf, and some other New Acquisitions. (Catalogue 70.) Pp. 54. (London: E. P. Goldschmidt and Co., Ltd.)

Methyl-Isothiourea B.D.H. Pp. 2. (London: The British Drug Houses, Ltd.)